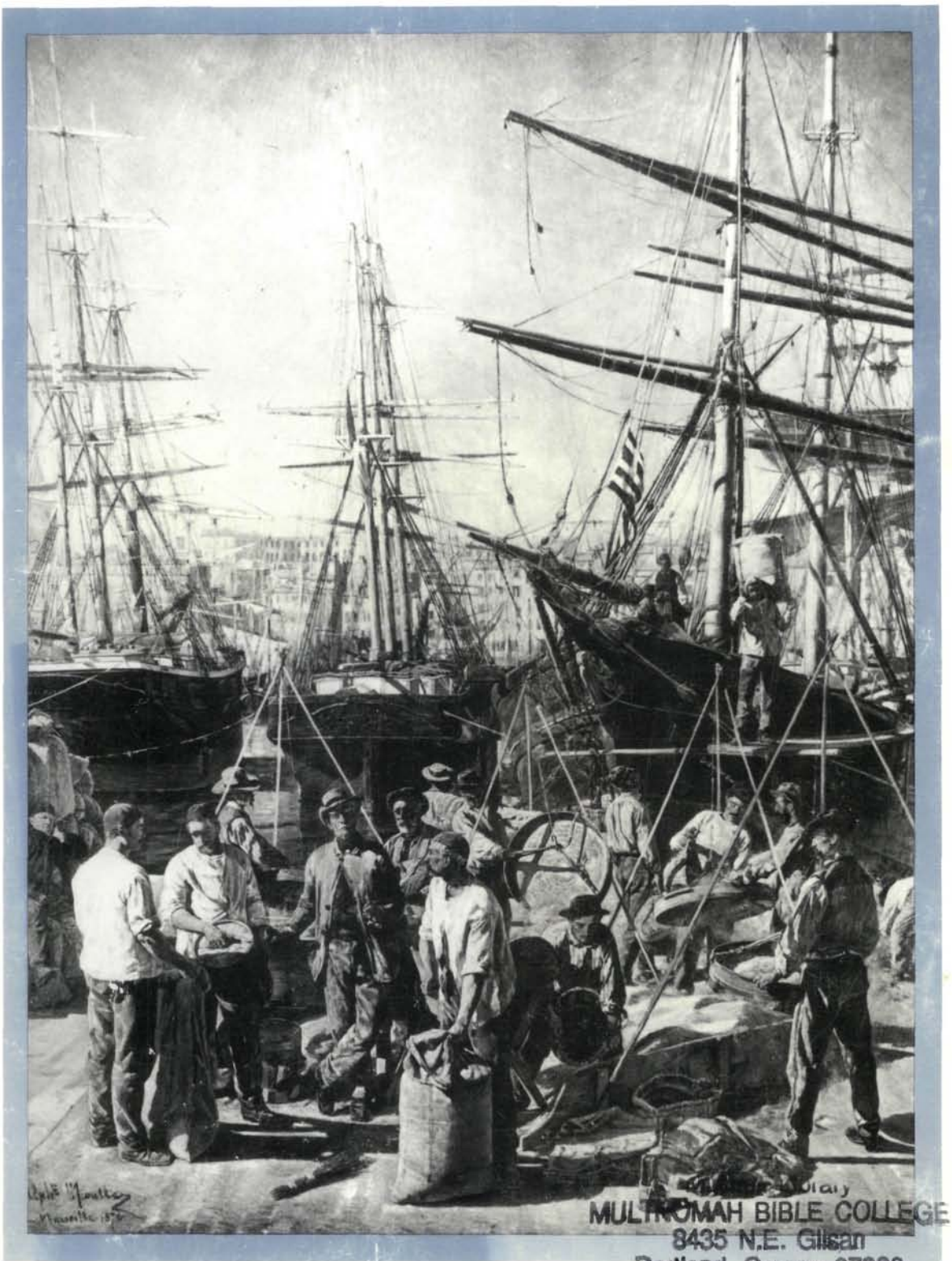
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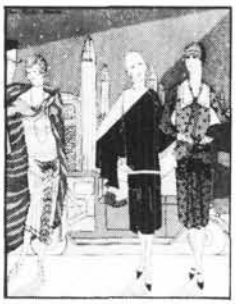
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Cover illustration: A team of dockworkers unloading grain in the old port in 1876. The master is standing with crossed legs and smoking a pipe. Unloading of grains in the old port continued to be undertaken by traditionally organized teams of dockworkers even in the 1870s, after the general monopoly of the dockworkers' society had been broken. "Débarquement de blé sur le quai de Rive-Neuve," by Alphonse Moutte. Courtesy of the Musée du Vieux-Marseille. See the article in this issue by William H. Sewell, Jr., "Uneven Development, the Autonomy of Politics, and the Dockworkers of Nineteenth-Century Marseille."

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In This Issue

This issue begins with an *AHR Forum* on questions of evidence, method, and morality. The text under examination is one of the most widely read recent works of historical scholarship, *The Return of Martin Guerre*, by Natalie Zemon Davis. **Robert Finlay** questions the adequacy of the evidence for Davis's strikingly original interpretation of this story of a sixteenth-century French peasant woman and the impostor who lived with her pretending to be her husband. Finlay contends that, in portraying the woman, Bertrande de Rols, not as a dupe but as a heroic collaborator of the false Martin Guerre, Davis goes beyond the evidence and substitutes assertion and intuition for the documentary record. Her version requires an unwarranted psychological reading of the principal source, a retelling of the story by one of the judges in the trial of the impostor. Finlay argues that the final product of the interpretive methods employed by Davis bears greater resemblance to historical romance than to a scholarly work of historical reconstruction.

Natalie Zemon Davis responds in a point-by-point rebuttal of the accusations made by Finlay. Interwoven with this defense of her methods and conclusions is a discussion of the differences between her and her accuser in regard to the purpose of retelling the story of these French peasants and, by extension, the purpose of any historical reconstruction. In taking the readers back to the sources, Davis combines contextual arguments (what we can understand by knowing more about peasants in Languedoc) and psychological arguments (what is typical of the human condition), and explains how historians can surmount the apparent limitations of a source such as the memoir of the judge that forms the basis of the Martin Guerre story.

Readers may be interested to know that fifty-three years ago the scholarship of another president of the AHA was subjected to scrutiny in the pages of the *AHR* in an exchange that raised some of the same issues as the current one. See the essay by Theodore Clarke Smith, "The Writing of American History in America, from 1884 to 1934" (*AHR*, 40 [1935]: 439–49) and the response by President Charles A. Beard, "That Noble Dream" (*AHR*, 41 [1935]: 74–87).

William H. Sewell, Jr., uses the dockworkers of Marseille in the nineteenth century as an example of the uneven development that was a common feature in the history of capitalism. This characteristic has frustrated predictions that social conditions would unify the working class in a political movement capable of overthrowing the capitalist order. The dockworkers enjoyed extensive privileges and operated under the protection of a worker-run corporation, despite laws against trade unions. Their special position (until the reorganization of dock work in the 1860s) illustrates the diversity of the working class and the problems of creating a unified movement of political opposition.

Mark Traugott examines the social identity of participants in the French Revolution of 1848. Using records of the Commission on National Compensation, he shows that the majority of insurgents in February were skilled artisans and broadly representative of the Parisian population as a whole. He compares the social characteristics of these insurgents to characteristics of insurgents in other uprisings, most notably the participants in the July Revolution of 1830 described by David Pinkney in an article in the *AHR* in 1964. Parallels in the preconditions and participants of these upheavals suggest that skilled artisans predominated in all major French insurrections of the nineteenth century.

Marjorie Murphy reviews four new books on the history of women and men in a variety of work settings. The studies demonstrate that, while cultural stereotypes and sex segregation were fostered by corporations to perpetuate profitable divisions of labor and by managers to control their work forces, the success of these strategies was limited in some settings. These new histories of working women analyze the complex play of power within the workplace and give lie to the stereotypes of women workers as meek and unwilling to take risks for financial gain.

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Contents

VOLUME 93 • NUMBER 3 • JUNE 1988

AHR Forum: The Return of Martin Guerre

The Refashioning of Martin Guerre
BY ROBERT FINLAY 553

"On the Lame"
BY NATALIE ZEMON DAVIS 572

Articles

Uneven Development, the Autonomy of Politics, and the Dockworkers of Nineteenth-Century Marseille
BY WILLIAM H. SEWELL, JR. 604

The Crowd in the French Revolution of February, 1848
BY MARK TRAUGOTT 638

Review Article

What Women Have Wrought
BY MARJORIE MURPHY 653

Reviews of Books

GENERAL

CHRISTA JUNGNICKEL and RUSSELL MCCORMMACH. *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein.* Volume 1, *The Torch of Mathematics, 1800–1870*; volume 2, *The Now Mighty Theoretical Physics, 1870–1925.* By I. Bernard Cohen 664

JOHN G. BURKE. *Cosmic Debris: Meteorites in History.* By Stephen G. Brush 665

RONALD L. NUMBERS, editor. *Medicine in the New World: New Spain, New France, and New England.* By J. Worth Estes 665

K. DAVID PATTERSON. *Pandemic Influenza, 1700–1900: A Study in Historical Epidemiology.* By Philip D. Curtin 666

MARTINE SEGALEN. *Historical Anthropology of the Family.* By Caroline B. Brettell 666

JACK GOODY. *The Logic of Writing and the Organization of Society.* By Kenneth Levine 667

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ABEL POITRINEAU. *Les mythologies révolutionnaires: L'utopie et la mort.* By Peter N. Stearns 668

LUCIANO PELLICANI. *Miseria del marxismo: Da Marx al Gulag.* By Alexander de Grand 668

EDOARDO TORTAROLO. *Illuminismo e rivoluzioni: Biografia politica di Filippo Mazzei.* By Emiliana P. Noether 669

GREGORY U. RIGSBY. *Alexander Crummell: Pioneer in Nineteenth-Century Pan-African Thought.* By Sylvia M. Jacobs 670

MARIANNE KRÜLL. *Freud and His Father.* By Gerald N. Izenberg 671

PHILLIS GROSSKURTH. *Melanie Klein: Her World and Her Work.* By Michael S. Roth 671

RICHARD NED LEBOW. *Nuclear Crisis Management: A Dangerous Illusion.* By Robert M. Hathaway 672

ANCIENT

ISRAEL EPH'AL. *The Ancient Arabs: Nomads on the Borders of the Fertile Crescent, 9th–5th Centuries B.C.* By Giorgio Buccellati 673

EVA CANTARELLA. *Pandora's Daughters: The Role and Status of Women in Greek and Roman Antiquity.* By Sarah B. Pomeroy 674

ERALDA NOE. *Storiografia imperiale pretacitiana: Linee di svolgimento.* By Herbert W. Benario 675

STEVEN E. SIDEBOTHAM. *Roman Economic Policy in the Erythra Thalassa, 30 B.C.–A.D. 217.* By J. A. S. Evans 675

MEDIEVAL

MARTYN J. WHITTOCK. *The Origins of England, 410–600.* By William A. Chaney 676

J. A. BURROW. *The Ages of Man: A Study in Medieval Writing and Thought.* By Penny S. Gold 677

MARTHA C. HOWELL. *Women, Production, and Patriarchy in Late Medieval Cities.* By Suzanne Fonay Wemple 677

MODERN EUROPE

LINDA FREY and MARSHA FREY. *Societies in Upheaval: Insurrections in France, Hungary, and Spain in the Early Eighteenth Century*; J. H. SHENNAN. *Liberty and Order in Early Modern Europe: The Subject and the State, 1650–1800.* By Marc Raeff 678

ROGER CHARTIER, editor. *Les usages de l'imprimé (XV^e–XIX^e siècle).* By Harry C. Payne 679

ARTUR ATTMAN. *American Bullion in the European World Trade, 1600–1800.* By John Jay TePaske 680

ALAN PALMER. *The Banner of Battle: The Story of the Crimean War.* By Ann Pottinger Saab 680

CHRISTIANE EISENBERG. *Deutsche und englische Gewerkschaften: Entstehung und Entwicklung bis 1878 im Vergleich.* By Eric Dorn Brose 681

ADAM PRZEWORSKI and JOHN SPRAGUE. *Paper Stones: A History of Electoral Socialism.* By Albert S. Lindemann 682

ARCADIUS KAHAN. *Essays in Jewish Social and Economic History.* By Shlomo Lambroza 682

ALEXANDER J. DE GRAND. *In Stalin's Shadow: Angelo Tasca and the Crisis of the Left in Italy and France, 1910–1945.* By Leo A. Loubere 683

J. M. BRERETON. *The British Soldier: A Social History from 1661 to the Present Day.* By Gordon C. Bond 684

C. G. A. CLAY. *Economic Expansion and Social Change: England, 1500–1700.* Volume 1, *People, Land, and Towns*; volume 2, *Industry, Trade, and Government.* By Frank T. Melton 685

G. R. ELTON. *The Parliament of England, 1559–1581.* By Dale Hoak 686

MARK A. KISHLANSKY. *Parliamentary Selection: Social and Political Choice in Early Modern England.* By Howard Nenner 686

DONALD DEAN SMEETON. *Lollard Themes in the Reformation Theology of William Tyndale.* By Anne Hudson 687

J. C. DAVIS. *Fear, Myth, and History: The Ranters and the Historians.* By Leo F. Solt 688

C. W. BROOKS. *Pettyfoggers and Vipers of the Commonwealth: The "Lower Branch" of the Legal Profession in Early Modern England.* By Brian P. Levack 689

CHRISTOPHER HILL. *The Collected Essays of Christopher Hill.* Volume 3, *People and Ideas in Seventeenth-Century England.* By Harvey J. Kaye 690

K. H. D. HALEY. *An English Diplomat in the Low Countries: Sir William Temple and John De Witt, 1665–1672.* By Stephen B. Baxter 691

IAN K. STEELE. *The English Atlantic, 1675–1740: An Exploration of Communication and Community.* By Daniel A. Baugh 692

C. H. LEE. *The British Economy since 1700: A Macroeconomic Perspective.* By Charles K. Hyde 693

SYBIL WOLFRAM. *In-Laws and Outlaws: Kinship and Marriage in England.* By Eileen Spring 693

TERRY CASTLE. *Masquerade and Civilization: The Carnivalesque in Eighteenth-Century English Culture and Fiction.* By Anya Peterson Royce 694

N. A. M. RODGER. *The Wooden World: An Anatomy of the Georgian Navy.* By Julian Gwyn 695

WHITNEY R. D. JONES. *David Williams: The Anvil and the Hammer.* By J. A. W. Gunn 696

BIANCAMARIA FONTANA. *Rethinking the Politics of Commercial Society: The* Edinburgh Review, *1802–1832.* By John Clive 697

NEVILLE THOMPSON. *Wellington after Waterloo.* By James J. Sack 697

RONALD K. HUCH and PAUL R. ZIEGLER. *Joseph Hume: The People's M.P.* By Thomas J. Spinner, Jr. 698

NICHOLAS C. EDSALL. *Richard Cobden: Independent Radical.* By Ronald K. Huch 699

WENDY HINDE. *Richard Cobden: A Victorian Outsider.* By Richard W. Davis 700

BERNARD SEMMEL. *John Stuart Mill and the Pursuit of Virtue.* By Ann Robson 700

MARTIN DOUGHTY, editor. *Building the Industrial City.* By Alexander Welsh 701

DONALD M. LEWIS. *Lighten Their Darkness: The Evangelical Mission to Working-Class London, 1828–1860.* By Jeffrey Cox 702

PETER R. H. SLEE. *Learning and a Liberal Education: The Study of Modern History in the Universities of Oxford, Cambridge, and Manchester, 1800–1914.* By Reba N. Soffer 702

RICHARD SYMONDS. *Oxford and Empire: The Last Lost Cause?* By John Kendle 703

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ELECTRONIC REPRODUCTION PROHIBITED

ERIC TAPLIN. *The Dockers' Union: A Study of the National Union of Dock Labourers, 1889–1922.* By Trygve R. Tholfsen 705

DAVID C. SMITH. *H. G. Wells: Desperately Mortal, A Biography.* By George Feaver 706

A. R. PETERS. *Anthony Eden at the Foreign Office, 1931–1938.* By James B. Christoph 707

OWEN CHADWICK. *Britain and the Vatican during the Second World War.* By Thomas E. Hachey 707

JAMES CABLE. *The Geneva Conference of 1954 on Indochina.* By Donald S. Birn 708

HELEN KELSALL and KEITH KELSALL. *Scottish Lifestyle 300 Years Ago: New Light on Edinburgh and Border Families.* By C. John Sommerville 709

T. C. SMOUT. *A Century of the Scottish People, 1830–1950.* By Bruce P. Lenman 709

JAMES LOUGHLIN. *Gladstone, Home Rule, and the Ulster Question: 1882–93.* By Lawrence J. McCaffrey 710

TREVOR WEST. *Horace Plunkett: Co-operation and Politics, an Irish Biography.* By Virginia E. Glandon 711

CLARE O'HALLORAN. *Partition and the Limits of Irish Nationalism: An Ideology under Stress.* By John D. Fair 712

JEAN-PIERRE BABELON. *Paris au XVI^e siècle.* By Barbara B. Diefendorf 713

DENISE TURREL. *Bourg-en-Bresse au XVI^e siècle: Les hommes et la ville.* By James L. Goldsmith 713

MICHEL ANTOINE. *Le dur métier de Roi: Etudes sur la civilisation politique de la France d'Ancien Régime.* By Sharon Kettering 714

GAY L. GULLICKSON. *Spinners and Weavers of Auffay: Rural Industry and the Sexual Division of Labor in a French Village, 1750–1850.* By Louise Tilly 715

RAN HALEVI. *Les loges maçonniques dans la France d'Ancien Régime: Aux origines de la sociabilité démocratique.* By Carolyn C. Lougee 716

ROBERT D. HARRIS. *Necker and the Revolution of 1789.* By Louis S. Greenbaum 716

R. R. PALMER, editor and translator. *The Two Tocquevilles, Father and Son: Hervé and Alexis de Tocqueville on the Coming of the French Revolution.* By Edward T. Gargan 717

DONALD E. ENGLISH. *Political Uses of Photography in the Third French Republic, 1871–1914.* By Eugen Weber 718

PEGGY A. PHILLIPS. *Modern France: Theories and Realities of Urban Planning.* By Anthony Sutcliffe 719

SHERRIN MARSHALL. *The Dutch Gentry, 1500–1650: Family, Faith, and Fortune.* By David Nicholas 719

GUNNAR ARTEUS. *Till Militärstatens Förhistoria: Krig, professionalisering och social förändring under Vasasönernas regering* [The Prehistory of the Military State: War, Professionalization, and Social Change under the Vasas]. By Heinz E. Ellersieck 720

ULRICH LANGE. *Landtag und Ausschuss: Zum Problem der Handlungsfähigkeit landständischer Versammlungen im Zeitalter der Entstehung des frühmodernen Staates; Die welfischen Territorien als Beispiel (1500–1629).* By Hanns Gross 721

ALOYS WINTERLING. *Der Hof der Kurfürsten von Köln 1688–1794: Eine Fallstudie zur Bedeutung "absolutischer" Hofhaltung.* By Roger Wines 721

SISKO HAIKALA. *"Britische Freiheit" und das Englandbild in der öffentlichen deutschen Diskussion im ausgehenden 18. Jahrhundert.* By T. C. W. Blanning 722

FERDINAND MAGEN. *Protestantische Kirche und Politik in Bayern: Möglichkeiten und Grenzen in der Zeit von Revolution und Reaktion 1848–1859.* By Jonathan Sperber 722

SUSANNE MILLER and HEINRICH POTTHOFF. *A History of German Social Democracy from 1848 to the Present.* By Gary P. Steenson 723

PETER BERGMANN. *Nietzsche, "the Last Antipolitical German."* By Roderick Stackelberg 724

HENNING KÖHLER. *Adenauer und die rheinische Republik: Der erste Anlauf 1918–1924.* By Robert P. Grathwol 725

RUDY KOSHAR. *Social Life, Local Politics, and Nazism: Marburg, 1880–1935.* By Johnpeter H. Grill 725

SIMON TAYLOR. *Prelude to Genocide: Nazi Ideology and the Struggle for Power.* By Jackson Spielvogel 726

CLAUDIA KOONZ. *Mothers in the Fatherland: Women, the Family, and Nazi Politics.* By Jill Stephenson 727

DETLEV J. K. PEUKERT. *Inside Nazi Germany: Conformity, Opposition, and Racism in Everyday Life.* By Rudy Koshar 728

GEORG LILIENTHAL. *Der "Lebensborn e. V.": Ein Instrument nationalsozialistischer Rassenpolitik.* By Larry V. Thompson 729

RICHARD PLANT. *The Pink Triangle: The Nazi War against Homosexuals.* By George M. Kren 729

STEPHEN PADGETT and TONY BURKETT. *Political Parties and Elections in West Germany: The Search for a New Stability.* By James H. Wolfe 730

R. BURR LITCHFIELD. *Emergence of a Bureaucracy: The Florentine Patricians, 1530–1790.* By Samuel Cohn, Jr. 731

JUDITH C. BROWN. *Immodest Acts: The Life of a Lesbian Nun in Renaissance Italy.* By Edward Muir 731

GIUSEPPE DE LUTIIS. *Storia dei servizi segreti in Italia.* By Elisa Carrillo 732

GUILLAUME DE BERTIER DE SAUVIGNY. *Metternich.* By Paul W. Schroeder 732

KARL HEINK STREITER. *Die nationalen Beziehungen im Grossherzogtum Posen (1815–1848).* By William W. Hagen 733

STEVEN BELA VARDY. *Baron Joseph Eötvös: A Literary Biography.* By Gabor Vermes 734

ENDRE KISS. *Der Tod der k.u.k. Weltordnung in Wien: Ideengeschichte Österreichs um die Jahrhundertwende.* By William M. Johnston 734

NEAL PEASE. *Poland, the United States and the Stabilization of Europe, 1919–1933.* By Robert Blobaum 735

CHARLES GATI. *Hungary and the Soviet Bloc.* By William O. McCagg, Jr. 736

N. S. BORISOV. *Russkaia tserkov' v politicheskoi bor'be XIV–XV vekov* [The Russian Church and the Political Struggles of the Fourteenth and Fifteenth Centuries]. By Nancy Shields Kollmann 737

JAMES F. BRENNAN. *Enlightened Despotism in Russia: The Reign of Elisabeth, 1741–1762.* By J. T. Alexander 737

HUGH D. HUDSON, JR. *The Rise of the Demidov Family and the Russian Iron Industry in the Eighteenth Century.* By Robert E. Jones 738

ROBERT H. MCNEAL. *Tsar and Cossack, 1855–1914.* By Paul Avrich 738

BERNICE GLATZER ROSENTHAL, editor. *Nietzsche in Russia.* By William Richardson 739

WILLIAM RICHARDSON. Zolotoe Runo *and Russian Modernism: 1905–1910.* By Bernice Glatzer Rosenthal 740

JANE BURBANK. *Intelligentsia and Revolution: Russian Views of Bolshevism, 1917–1922.* By Barbara Evans Clements 741

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CATHERINE ANDREYEV. *Vlasov and the Russian Liberation Movement: Soviet Reality and Emigré Theories.* By Ralph T. Fisher, Jr. 744

NEAR EAST

HABIB BEN ABDALLAH. *De l'iqta' étatique a l'iqta' militaire: Translation économique et changements sociaux à Baghdad, 247–447 de l'Hégire, 861–1055 ap. J.* By Michael G. Morony 744

CORNELL H. FLEISCHER. *Bureaucrat and Intellectual in the Ottoman Empire: The Historian Mustafa Ali (1541–1600).* By I. Metin Kunt 745

JOHN ROBERT BARNES. *An Introduction to Religious Foundations in the Ottoman Empire.* By Rudi Paul Lindner 746

FRANCIS DESRAMAUT. *L'Orphelinat Jésus-Adolescent de Nazareth en Galilée: Au temps des Turcs, puis des Anglais (1896–1948).* By Raphael Patai 746

ISRAEL GERSHONI and JAMES P. JANKOWSKI. *Egypt and the Arabs: The Search for Egyptian Nationhood, 1900–1930.* By Daniel Crecelius 747

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AFRICA

STEPHEN G. BUNKER. *Peasants against the State: The Politics of Market Control in Bugisu, Uganda, 1900–1983.* By Robert M. Maxon 748

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ANDREW PORTER. *Victorian Shipping, Business, and Imperial Policy: Donald Currie, the Castle Line, and Southern Africa.* By Robert Kubicek 750

ASIA AND THE EAST

STANLEY WEINSTEIN. *Buddhism under the T'ang.* By Daniel L. Overmyer 751

RICHARD J. SMITH. *China's Cultural Heritage: The Ch'ing Dynasty, 1644–1912.* By Lynn Struve 751

JAMES H. COLE. *Shaohsing: Competition and Cooperation in Nineteenth-Century China.* By Jonathan K. Ocko 752

MARY BACKUS RANKIN. *Elite Activism and Political Transformation in China: Zhejiang Province, 1865–1911.* By Suzanne Wilson Barnett 753

JOHN KING FAIRBANK. *The Great Chinese Revolution, 1800–1985.* By James E. Sheridan 754

MICHAEL A. BARNHART. *Japan Prepares for Total War: The Search for Economic Security, 1919–1941.* By Thomas R. H. Havens 755

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UNITED STATES

DAVID BRION DAVIS. *From Homicide to Slavery: Studies in American Culture.* By Winthrop D. Jordan 757

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JOHN S. GILKESON, JR. *Middle-Class Providence, 1820–1940.* By Karen Halttunen 762

JOHN E. FINDLING. *Close Neighbors, Distant Friends: United States–Central American Relations.* By Lester D. Langley 763

HILLEL SCHWARTZ. *Never Satisfied: A Cultural History of Diets, Fantasies, and Fat.* By Michael Fellman 763

TERESA TOULOUSE. *The Art of Prophesying: New England Sermons and the Shaping of Belief.* By Francis J. Bremer 764

PATRICIA U. BONOMI. *Under the Cope of Heaven: Religion, Society, and Politics in Colonial America.* By Catherine L. Albanese 765

ALAN VANCE BRICELAND. *Westward from Virginia: The Exploration of the Virginia-Carolina Frontier, 1650–1710.* By James H. O'Donnell III 765

EDWARD BYERS. *The Nation of Nantucket: Society and Politics in an Early American Commercial Center, 1660–1820.* By Bruce C. Daniels 766

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RICHARD B. MORRIS. *The Forging of the Union, 1781–1789.* By Peter S. Onuf 768

DANIEL GEORGE LANG. *Foreign Policy in the Early Republic: The Law of Nations and the Balance of Power.* By Clifford Egan 769

WILLIAM G. MCLOUGHLIN. *Cherokee Renascence in the New Republic.* By Gary E. Moulton 769

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ELECTRONIC REPRODUCTION PROHIBITED

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DAVID SCHUYLER. *The New Urban Landscape: The Redefinition of City Form in Nineteenth-Century America.* By Richard E. Foglesong 778

W. BRUCE FYE. *The Development of American Physiology: Scientific Method in the Nineteenth Century.* By Peter C. English 778

CINDY SONDIK ARON. *Ladies and Gentlemen of the Civil Service: Middle-Class Workers in Victorian America.* By Lynn Y. Weiner 779

CYNTHIA GRIFFIN WOLFF. *Emily Dickinson.* By Barbara Welter 780

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DOUGLAS FRANK. *Less Than Conquerors: How Evangelicals Entered the Twentieth Century.* By David Edwin Harrell, Jr. 781

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CANADA

A. A. DEN OTTER. *Civilizing the West: The Galts and the Development of Western Canada.* By Dianne Newell 798

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LATIN AMERICA

LESLIE BETHELL, editor. *The Cambridge History of Latin America.* Volume 4, *c. 1870 to 1930*; LESLIE BETHELL, editor. *The Cambridge History of Latin America.* Volume 5, *c. 1870 to 1930.* By Peter Winn 799

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RHYS JENKINS. *Transnational Corporations and the Latin American Automobile Industry.* By Kevin J. Middlebrook 802

RONALD KENT RICHARDSON. *Moral Imperium: Afro-Caribbeans and the Transformation of British Rule, 1776–1838.* By Seymour Drescher 803

LOUIS A. PEREZ, JR. *Cuba under the Platt Amendment, 1902–1934.* By Jorge I. Domínguez 803

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THOMAS R. MCGUIRE. *Politics and Ethnicity on the Rio Yaqui: Potam Revisited.* By Stuart F. Voss 808

LINDA NEWSON. *The Cost of Conquest: Indian Decline in Honduras under Spanish Rule.* By Christopher H. Lutz 809

E. BRADFORD BURNS. *Eadweard Muybridge in Guatemala, 1875: The Photographer as Social Recorder.* By Lowell Gudmundson 810

Collected Essays 812

Documents and Bibliographies 820

Other Books Received 821

Communications 825

Index of Advertisers 48(a)

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Contributors

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As appeared in *The Return of Martin Guerre,* a courtroom scene, confrontation between accused and witness. From Jean Milles de Souvigny, *Praxis Criminis Persequendi* (Paris, 1541). Courtesy of the Harvard Law School Library, Treasure Room.

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AHR Forum: The Return of Martin Guerre

The Refashioning of Martin Guerre

ROBERT FINLAY

WHILE MOST RENAISSANCE POPES AND PRINCES HAVE BEEN FORGOTTEN by everyone but the historical specialist, one peasant of the sixteenth century, from a village near Toulouse in the foothills of the Pyrenees, remains well known. Martin Guerre—or rather the impostor who took his wife and birthright—has entered history. This is a remarkable fact, for generally the world of peasants lay outside what the elite of Europe in the past considered significant. Peasants were viewed within the comforting contexts of proverbial wisdom and pastoral buffoonery.[1] The personalities and perspectives of rural people usually were recorded only when peasants ran into trouble with the law. Emmanuel Le Roy Ladurie's reconstruction of rural life in southern France in the fourteenth century and Carlo Ginzburg's examination of peasant religious beliefs in sixteenth-century Friuli rely on legal records.[2] These works have been acclaimed for revealing the motivations and values of ordinary people of the past, hitherto rendered mute by both their illiteracy and ignoble status.

Even within this select company, however, Natalie Zemon Davis's *Return of Martin Guerre* is exceptional. The events she examines were never lost to sight but instead became famous immediately and eventually inspired a play, two novels, and an operetta. The story was irresistible: the impostor, Arnaud du Tilh, posed as Martin Guerre, the husband of Bertrande de Rols, for over three years, thereby gaining a wife and property and fathering a child, only to be exposed by the true husband when he was on the verge of refuting those accusing him of deception. While Davis's book carried this dramatic story to an English-speaking audience, a film, *Le Retour de Martin Guerre*, the product of collaboration between Davis and the moviemakers, presented it to an international one. No doubt because of the excellence and wide distribution of the film, Davis's book reached a more extensive

I would like to thank Elizabeth Anne Payne and Donald E. Queller for their encouragement and criticism in the writing of this essay.

[1] Natalie Zemon Davis, *The Return of Martin Guerre* (Cambridge, Mass., 1983), 1–2.

[2] Emmanuel Le Roy Ladurie, *Montaillou: The Promised Land of Error*, trans. Barbara Bray (New York, 1978); Carlo Ginzburg, *The Cheese and the Worms: The Cosmos of a Sixteenth-Century Miller*, trans. John Tedeschi and Anne Tedeschi (Baltimore, Md., 1980); see also his *The Night Battles: Witchcraft and Agrarian Cults in the Sixteenth and Seventeenth Centuries*, trans. John Tedeschi and Anne Tedeschi (Baltimore, Md., 1983).

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audience than does the usual historical study. It has been hailed in the popular press as a vivid supplement to the film and in academic journals as a "realistic and brilliantly scholarly monograph," an "imaginative history which is nevertheless solidly based and intelligently argued," a "major work of historical reconstruction . . . performed without any kind of ideological bias."[3] It is the consensus, then, that *The Return of Martin Guerre* is a genuine rarity, a work of sophisticated scholarship with general appeal, a study that remains faithful to academic standards while conveying all the color and drama of a famous tale.

DAVIS WAS DRIVEN TO GIVE THE STORY OF MARTIN GUERRE its "first full-scale historical treatment" because she came to feel that the film did not adequately address "the motivations of people in the sixteenth century." She was troubled that "the film was departing from the historical record," especially because "the double game of the wife and the judge's inner contradictions were softened." She wanted to make room for the "perhapses" and "may-have-beens" that the historian uses to explain inadequate and perplexing evidence. She tells her readers that "what I offer you here is in part my invention, but held tightly in check by the voices of the past."[4] The inventive aspects of Davis's book stem largely from her employment of concepts and methods drawn from recent innovations in anthropology, ethnography, and literary criticism, all fields that have had a significant influence on what is widely regarded as the best contemporary historical scholarship.[5] The result of Davis's labors is a reinterpretation of the Martin Guerre story that is imaginatively conceived, eloquently argued, and intrinsically appealing. It is also strikingly different from the version of the story that has been accepted since the sixteenth century.

The traditional version of the story of Martin Guerre derives from accounts of the sixteenth century, especially from Jean de Coras's *Arrest Memorable*, written by the *rapporteur* at the trial and a judge of the Parlement of Toulouse after the pseudo-husband was executed in 1560.[6] In Coras's commentary, the main character was Arnaud du Tilh, also known as Pansette, "the belly," a charlatan brought to disaster by his own cunning and ambition. "It was truly a tragedy for this fine peasant," the judge wrote, "all the more because the outcome was wretched, indeed fatal for him."[7] In his narrative as well as in his courtroom, Coras

[3] The quotations are, respectively, from reviews by A. Lloyd Moote, *AHR*, 90 (October 1985): 943; Donald R. Kelley, *Renaissance Quarterly*, 37 (1984): 252; and Emmanuel Le Roy Ladurie, *New York Review of Books*, 30 (22 December 1983): 12–14. See also reviews by William Monter, *Sixteenth Century Journal*, 14 (1983): 516; Edward Benson, *French Review*, 57 (1984): 753–54; Michel Simonin, *Bibliothèque d'Humanisme et Renaissance*, 47 (1985): 286–87; R. J. Knecht, *History*, 70 (1985): 121.

[4] Davis, *Return of Martin Guerre*, viii–ix, 5.

[5] For example, see the works cited in note 2, above.

[6] Another contemporary work was Guillaume Le Sueur's *Admiranda historia de Pseudo Martino Tholosae*, a pamphlet following the genre of a news account. Davis's reinterpretation of the Martin Guerre story depends on her reading of Coras's *Arrest Memorable*, and she gives the latter far greater weight than Le Sueur's shorter, simpler account (see Davis, *Return of Martin Guerre*, 4–5, 104, 114–15, 153 n. 17).

[7] Davis, *Return of Martin Guerre*, 111.

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unequivocally condemned Arnaud, yet, as Davis shows, he had a certain admiration for the impostor's abilities. Coras's focus was on the marvelous deception perpetrated by Arnaud, and in the many subsequent retellings of the tale, the emphasis was similarly on the arch-trickster, the sly thief of sexual favors and property.

Davis presents a radically different interpretation in which the focus is on Bertrande de Rols or, rather, on her relationship with the impostor. According to Davis, Bertrande was in fact Arnaud's accomplice, for she knew that the man claiming to be her husband was a fraud. She accepted Arnaud, they fell in love, and they regarded themselves as having an "invented" marriage. They willfully fabricated a lie, and, when challenged in court, they concocted a strategy of deceit and manipulation: "Bertrande searched her memory for a sexual episode [involving her true husband]—perhaps even embroidered it—with which they could surprise the court."[8] With the return of Martin Guerre, however, Bertrande's "double role" collapsed, and she brought forth "prepared excuses" for her conduct. For his part, Arnaud remained faithful to his lover and accomplice, asserting that she had been duped as thoroughly as her fellow villagers of Artigat. Even in his confession on the way to the gibbet, the impostor "concealed from start to finish" Bertrande's role in their elaborate collusion.[9]

These two versions of the story of Martin Guerre could hardly be more different. The traditional account is a narrative of greed and deception, of perverted talents and a duped woman, of great ability in the service of fraud and theft. Davis's book tells a tale of devotion and collaboration, of love and identity, of how an invented marriage was destroyed by a hard-hearted man with a wooden leg. To Coras, Arnaud's abilities—his quick tongue and retentive memory—led a fine peasant into a tragicomedy of imposture. To Davis, the tragedy lies in the unmasking of Arnaud, "a kind of hero, a more real Martin Guerre" than the unsympathetic husband of Bertrande de Rols.[10]

The sharpest contrast between the two versions is in the characterization of Bertrande. In Coras's eyes, she was a dupe, who, "given the weakness of her sex, [was] easily deceived by the cunning and craftiness of men."[11] He considered her ignorant of Arnaud's true identity, hence innocent of wrongdoing. The Criminal Chamber of the Parlement of Toulouse agreed and declared that her child fathered by Arnaud must be regarded as legitimate. In his confession, Arnaud begged Bertrande's pardon for cruelly deceiving her. Amid all the squabbles in Artigat and in all the testimony taken from 180 witnesses by the court, apparently no one ever suggested that Bertrande was Arnaud's accomplice.

No one, that is, until Davis and *The Return of Martin Guerre*.[12] According to Davis,

[8] Davis, *Return of Martin Guerre*, 57; 44, 50, 68–69.
[9] Davis, *Return of Martin Guerre*, 69, 86, 92.
[10] Davis, *Return of Martin Guerre*, 113.
[11] Davis, *Return of Martin Guerre*, 110.
[12] Davis suggests that a hint of Bertrande as an accomplice—or what she styles "the self-fashioning Bertrande" (*Return of Martin Guerre*, 118)—is found in two "male responses" to the story of Martin Guerre. The writers that she cites, however, did not regard Bertrande as an accomplice. A French poet of the later sixteenth century merely expressed sympathy "with the tricked wife," while Montaigne, in

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Bertrande played a double role, deceiving her son by Martin Guerre, her relatives, friends, and neighbors. At the same time, Davis also turns upside down the moral judgment informing the traditional version of the story. No doubt if Bertrande's contemporaries believed she was guilty, they would have thought her as despicable as the impostor. From Davis's perspective, however, Bertrande is a heroic figure, independent, clear-sighted, passionate, and invariably "honorable." While Coras saw the wife as an innocent victim, Davis views her as a knowing actor and, though guilty of adultery and deception, all the more admirable because of the values embodied in her heroic transgression.

There is a complex relationship between these two versions of the Martin Guerre story. Davis does not simply counter Coras's sixteenth-century interpretation with her own twentieth-century one. Rather, in the absence of trial records, her analysis must depend on Coras's own account. In short, the twentieth-century reinterpretation presumably is based on a reevaluation of evidence contained in Coras's very different interpretation. With all its flaws, "combining features of a legal text and a literary tale,"[13] the *Arrest Memorable* remains the best source for the story of Martin Guerre, and Davis necessarily must reinterpret it to sustain her own version. Of course, Coras's text does not give privileged access to past reality; the judge's assumption of Bertrande's innocence is not proof that she was in fact innocent. Indeed, to the extent that Coras's assertion of Bertrande's innocence may have been a consequence of a low estimation of female intelligence and judgment, one might be tempted to equate a declaration of Bertrande's complicity with a rejection of the judge's benighted view of the female sex. Davis eschews that temptation, however, and one of the strengths of her book is its portrait of Jean de Coras as a thoughtful, humane scholar, fully capable of recognizing female intelligence and of looking beyond elitist and patriarchal prejudices in his pursuit of truth.

Davis situates Bertrande's supposed complicity within her view of sixteenth-century peasant society. From this perspective, Bertrande's conduct is seen as an instance of the ingenuity and calculation commonly shown by peasant women who must maneuver within a patriarchal system. The male-dominated society put a premium on "the woman's ability to get her way with the men and to calculate her advantages," an ability that women passed along "through the deep tie and hidden complicity of mother and daughter." The four sisters of Martin Guerre were apparently displaying typically feminine calculation in siding with the pseudo-husband against his accuser: "[T]hey may have preferred him to their uncle as head of the family and its property." In Artigat, Bertrande "tried to fashion her

his "Of the Lame," referred briefly to the trial for imposture but without mentioning either Bertrande or a wife (see Davis, 118–19). In a 1941 novel, Janet Lewis presents a Bertrande who tacitly accepts the impostor for years and only slowly comes to realize the implications of her action. After reading Coras much later, Lewis wrote that she would have approached the story differently, apparently because of the indications in Coras that Bertrande had been suborned by Pierre Guerre (Janet Lewis, "Sources of *The Wife of Martin Guerre*," *Triquarterly*, 55 [Fall 1982]: 104); for more on Lewis's novel, see note 73 below. The same volume of *Triquarterly* (86–103) contains a translation of the main text of *Arrest Memorable*.

[13] Davis, *Return of Martin Guerre*, 4.

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life as best she could, using all the leeway and imagination she had as a woman." During the trial of the impostor, "she had to manipulate the image of the woman-easily-deceived, a skill that women often displayed before officers of justice any time it was to their advantage."[14] As one reviewer of Davis's book summed it up, Bertrande almost succeeded in her "clever, dogged, double game of having her cake and eating it within the confines of a particularist, male-dominated society."[15] This interpretation has hitherto been missing from retellings of the story of Martin Guerre inasmuch as Bertrande has been reduced to playing a muted second fiddle to Arnaud, "the inventive figure in the tale." Bertrande's true role has been overlooked or suppressed, Davis suggests, because of the unfortunate circumstance "that we have no female commentary on the story until the twentieth century."[16]

However that may be, Davis fails to show that her view of women in peasant society is relevant to the case she is examining. Instead, she imposes her notion of peasant women on Bertrande, whose conduct and character thereby are seen as if Bertrande had regarded the pseudo-Martin with the same calculating, self-interested eye that was supposedly characteristic of peasant women in general. In other words, since Davis assumes that rustic women consistently and covertly maneuvered for their own advantage, she does not consider it necessary to justify her assertions about Bertrande's calculating behavior. But, whatever the accuracy of Davis's view of peasant society, her application of that perspective to the story of Martin Guerre does not yield a portrait of Bertrande that is either plausible or persuasive.

DAVIS ADDS SUBSTANTIALLY TO ESTABLISHING THE HISTORICAL CONTEXT within which the story should be understood when she uses archival material to illuminate questions of kinship relations, inheritance law, peasant migration, marriage contracts, military service, village custom, and judicial procedure. But, when she considers the relationship between Bertrande and Arnaud, the linchpin of her book, she is faced with a central difficulty: the historical record indicates that Bertrande was universally regarded as the impostor's victim, not his accomplice. In fact, the question of Bertrande's complicity never arose at the time, and Davis presents no evidence for her contention that "after much discussion" about Bertrande, the judges of Toulouse "agreed to accept her good faith" and not prosecute her for "fraud, bigamy, or adultery."[17] Bertrande's good faith was always assumed, never a matter of debate, and, precisely because she was never considered liable for those charges, she was not prosecuted. Davis's claim to the contrary is not based on newly discovered material or on examination of surviving

[14] Davis, *Return of Martin Guerre*, 31, 55, 60, 68.
[15] A. Lloyd Moote, *AHR*, 90 (October 1985): 943.
[16] Davis, *Return of Martin Guerre*, 118.
[17] Davis, *Return of Martin Guerre*, 90. On the same page, Davis states that the judges had to decide "what to do about the woman prisoner in the Conciergerie," that is, Bertrande. It is easy to forget at this point in Davis's narrative that Bertrande was imprisoned not on charges of fraud, bigamy, or adultery but because the judges suspected her of falsely accusing her husband of imposture.

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records. Instead, it depends on her mere assertion that she has recognized a truth that apparently remained hidden from both the villagers of Artigat and the judges of Toulouse.

> What of Bertrande de Rols? Did she know that the new Martin was not the man who had abandoned her eight years before? Perhaps not at the very first, when he arrived with all his "signs" and proofs. But the obstinate and honorable Bertrande does not seem a woman so easily fooled, not even by a charmer like Pansette. By the time she had received him in her bed, she must have realized the difference; as any wife of Artigat would have agreed, there is no mistaking "the touch of the man on the woman." Either by explicit or tacit agreement, she helped him become her husband.[18]

This is Davis's entire basis for claiming that the wife was in league with the impostor: that is, in sexual relations, Bertrande could not have failed to realize that her partner was not her true husband. Not only is there no hint of this in the sources, but the claim runs counter to the account Davis herself gives of Bertrande's dismal sexual experience: she was married for some nine years without intercourse, which was finally achieved after a magic spell was lifted; pregnancy took place immediately, and, when their son was several months old, Martin abandoned her for eight years.[19] After seventeen years of marriage, then, Martin and Bertrande could only have had sexual intercourse for a few months at most, hardly the sort of connubial experience making for a "touch of the man" that was indelible or unmistakable.

If Bertrande had perceived any difference in sexual manner between her spouse and the man claiming to be her husband, she could reasonably have explained it to herself as a consequence of her years of sexual abstinence, during which the precarious sexuality of her husband evidently gave way to virile confidence, perhaps assisted by the fleshpots of Spain and Picardy, where he soldiered. Equally important, Bertrande would have had to weigh any doubts arising from his sexual performance against the universal welcome accorded her presumed husband. Like all her in-laws and neighbors, Bertrande was fooled by Arnaud from the beginning.[20] He had gathered information about the Guerres' marriage before his appearance, and he had days with Bertrande before going to bed with her in which to become familiar and garner even more intimate knowledge.[21] He was welcomed home first by the sisters of Martin Guerre, whom Bertrande later especially blamed for misleading her. Coras gave substantial weight to the response of the sisters, which, along with the proofs of Arnaud and the wife's eagerness to have her husband back again, seemed to the judge wholly sufficient to explain Bertrande's falling for the impostor's lies.[22]

[18] Davis, *Return of Martin Guerre*, 43–44.
[19] Davis, *Return of Martin Guerre*, 19–21, 24.
[20] Davis, *Return of Martin Guerre*, 44, 61.
[21] Davis, *Return of Martin Guerre*, 42–43.
[22] Jean de Coras, *Arrest Memorable, du Parlement de Tolose, Contenant une histoire prodigieuse, de nostre temps, avec cent belles, & doctes Annotations, de monsieur maistre Jean de Coras, Conseiller en ladite Cour, & rapporteur du proces; Prononcé es Arrestz Generaulx le xii Septembre MDLX* (Lyon, 1561), 47, 52, 60–61, 67, 112. Davis states that Bertrande, when presented with her true husband at the trial, retailed her "prepared excuses: your sisters believed him too readily; your uncle accepted him"; 86. Coras's account,

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Davis offers nothing to counter Jean de Coras's understanding of Bertrande's innocence other than her own assertion about an inevitable sexual recognition, backed by a proverb about "the touch of the man." Even that proverb is not uniquely pertinent, for, in the conditions of sexual intercourse in peasant households of preindustrial Europe—the couple clothed, in a darkened house, amid the cold and dirt, surrounded by livestock and relatives—any wife of Artigat might equally well have regarded her bedmate with the jaded perspective expressed in the notorious masculine slur, "De nuit tous chats son gris."[23]

In any event, even Davis agrees that Bertrande accepted Arnaud in her marriage bed because she was certain he was her spouse. Surely, the impostor's subsequent sexual habits would have had to be extraordinary for the happy, trusting wife to conclude that she was in error. As far as one can tell, neither she nor anyone else at the time came to that conclusion. The assumption, then, that sexual relations must have revealed the imposture to Bertrande is not an interpretation based on the sources; it is, rather, an opinion by a modern historian who apparently believes that unsubstantiated insight can itself be taken as evidence. This is a flimsy foundation on which to build an interpretation of the Martin Guerre story that contradicts the surviving evidence.

Behind Davis's assertion about Bertrande's response to the impostor is an equally unfounded assessment of the wife's character. After quoting a late fifteenth-century work, the *Malleus Maleficarum*, to the effect that the devil can make a wife consider "her husband so loathsome that not for all the world would she allow him to lie with her," Davis states that "Bertrande might not have put it in these words, but it seems clear that for a while [after marrying Martin] she was relieved that they could not have intercourse." In fact, Davis presents no evidence that Bertrande had any such view. The assertion about intercourse, however, is of a piece with Davis's portrayal of Bertrande as a woman of "stubborn independence," one who considered the bewitchment afflicting her marriage lifted only when she "was ready for it." When urged to divorce her impotent mate, she refused because of her "shrewd realism about how she could maneuver within the constraints placed upon one of her sex."[24] While Coras viewed this refusal as a touchstone of Bertrande's integrity, Davis sees it as a manifestation of the same

however, is considerably more pointed about whom Bertrande blamed: "Accusant les seurs dudit Martin sur tous les autres, qui avoient trop facilement creu, et asseuré, que le prisonnier estoit Martin Guerre leur frère"; 81.

[23] In his commentary in the *Adages* on Plutarch's "All women are the same when the lights are out," Erasmus wrote, "Ego certe antequam Plutarchi locum adiissem, hujusce Graeci adagii sensum à Gallico edoctus eram adagio. *De nuict tous chats son gris*" (*Opera Omnia* [Leiden, 1703], 2: 821). On cats, sex, and "all cats are gray at night," see Robert Darnton, *The Great Cat Massacre and Other Episodes in French Cultural History* (New York, 1985), 95–96. On peasant sexuality, see Jean-Louis Flandrin, *Families in Former Times: Kinship, Household and Sexuality*, trans. Richard Southern (Cambridge, 1979); *Les Amours paysannes: Amour et sexualité dans les campagnes de l'ancienne France (XVI^e–XIX^e siècle)* (Paris, 1975). Some comments by Montaigne suggest that pre-modern notions of sexual passion in marriage were very different from contemporary ones: see Michel de Montaigne, "On Some Verses of Virgil," *The Complete Essays of Montaigne*, trans. Donald M. Frame (Stanford, Calif., 1965), 646–47, 649. See also Philippe Ariès, "Love in Married Life," *Western Sexuality: Practice and Precept in Past and Present Times*, Philippe Ariès and André Béjin, eds., Anthony Foster, trans. (Oxford, 1985), 131, 136–38.

[24] Davis, *Return of Martin Guerre*, 28.

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shrewd realism that led Bertrande knowingly to accept the pseudo-husband, connive with him in deceiving her kinfolk, and finally abandon him when Martin Guerre reappeared.[25]

According to Davis, Bertrande's realism and independence explain why the wife took her presumed husband to court, cooperating with Pierre Guerre, Martin's uncle, after he illegally acted as her agent in having Arnaud imprisoned for imposture. Explaining why Bertrande acted as a plaintiff on a charge of imposture against Arnaud is a crucial matter for Davis, since common sense dictates that an accomplice is unlikely to press a criminal charge against a collaborator. In the service of resolving this difficulty, Davis provides some questions that she supposes must have troubled Bertrande: "Would God punish them because of the lie? . . . She loved the new Martin, but he had tricked her once; might he after all not trick her again? And what if the other Martin Guerre came back?" Under pressure from her mother and Pierre Guerre (who was also her stepfather), "the stubborn woman calculated and made her plans. She would go along with the court case and hope to lose it. She would follow the strategy she had worked out with the new Martin about testimony and hope that the judge would declare him her husband . . . [S]he would also be prepared to win the case, however terrible the consequences for the new Martin."[26]

Evidently a creature of utter calculation, Bertrande thus pursued a shrewd strategy in court that she kept secret from both her accomplice and his accusers. In the odd position of being a plaintiff in collusion with the defendant, she had to be careful not to trip up her pseudo-husband, while also pretending to be completely honest with the judges. She had to appear to be easily deceived but without convincing the court that she was in fact a victim of deception.[27] She had to make an accusation that she knew to be true, while also plotting with her accomplice to expose herself as a false accuser. In Davis's view, then, the peasant woman from Artigat committed herself to a plan of dizzying subtlety and complexity, one involving extraordinary manipulation of her relatives, the judges, and even her lover.

Coras recorded that in court Bertrande was nervous and uncertain, trembling in speech and with eyes fixed on the floor. The deluded judge did not realize, Davis asserts, that this was all a clever act, that Bertrande was adhering to a "text" she and her accomplice "had agreed upon months before."[28] Nor did Coras recognize that Bertrande was acting when she started weeping and trembling upon being confronted with Martin Guerre: "She was sufficiently steeled to the different possible outcomes of her situation so that when she arrived at the Criminal Chamber she was able to play her role quite well."[29]

In Davis's interpretation, Pansette the impostor's role was also dauntingly intricate: to defend himself against the charge brought by Bertrande, he had to

[25] Coras, *Arrest Memorable*, 33; Davis, *Return of Martin Guerre*, 28.
[26] Davis, *Return of Martin Guerre*, 60–61.
[27] Davis, *Return of Martin Guerre*, 68–69.
[28] Davis, *Return of Martin Guerre*, 75–76, 80.
[29] Davis, *Return of Martin Guerre*, 85.

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show that his accomplice had been suborned and was thus guilty of making a false accusation. He was successful at this ploy, and the court ordered Bertrande (and Pierre Guerre) imprisoned during the trial;[30] hence, success for the impostor meant a serious criminal charge levied against his accomplice. Of course, given Davis's scenario of complicity, all that Bertrande had to do to avoid such a fate was to refuse to render herself as plaintiff against her pseudo-husband, an action that would have had the incidental benefit of removing her lover from threat of execution. Davis never explains why this obvious and safe course was not taken by the duplicitous couple.

Davis's scenario becomes bewilderingly complex with the return of Martin Guerre. Abandoning the accomplice she had charged with imposture, Bertrande perforce wins her case and pours forth "prepared excuses." Arnaud still insists that he is the true husband, but the judges do not put the obdurate liar to torture because "they certainly did not want him to name Bertrande de Rols as his accomplice at the last minute."[31] Davis fails to explain why the judges either suspected or covered up the peasant woman's supposed crimes. Moreover, Davis's argument requires us to believe that the judges who had imprisoned the plaintiff on suspicion of bringing a false accusation against her husband were also suspicious that the plaintiff was an accomplice in the imposture of the man she had charged with that crime. In other words, the court somehow suspected Bertrande of both false accusation and complicity with the accused. According to Davis, only one person suggested that Bertrande was not deceived by Arnaud: "Was the weakness of the [female] sex really so great that wives could not tell the difference between married love and adultery? The cuckolded Martin Guerre clearly thought not, as we know from the words attributed to him in court by both Coras and Le Sueur."[32] Yet it is clear from those words that Martin Guerre simply (albeit severely) rebuked his wife for imprudence, not for choosing adultery over marital fealty.[33] A cuckold is a man whose wife has committed adultery, and apparently no one at the trial regarded the returned husband in that light. The judges would hardly have urged him to compassion for Bertrande if they had thought he was accusing his wife of the very crime of which they allegedly suspected her.[34] In sum, Davis presents a scenario in which the principal participants in a trial—the plaintiff, the defendant, and the judges—harbored secret designs and motivations that, by their very nature, cannot be substantiated by the sources.

[30] Davis, *Return of Martin Guerre*, 68–69, 76.
[31] Davis, *Return of Martin Guerre*, 86, 90.
[32] Davis, *Return of Martin Guerre*, 110. Davis follows this with the statement that "it is hard to imagine that the Coras we have seen dealing with Jacquette de Bussi [his wife] could consistently believe that women were so easy to trick"; 110. The implication is that, since Coras had high regard for his wife's intelligence, he must somehow have seen Bertrande as undeceived by the impostor. Davis once more adduces Coras's wife to make the same point: "[W]e have no female commentary on the story until the twentieth century. Jacquette de Bussi's reaction to her husband's gift book [that is, the *Arrest Memorable*] is unrecorded. I doubt that she believed that Bertrande de Rols could have been deceived for so long"; 118. Davis's suggestion, then, is apparently that Jacquette de Bussi would have seen through her husband's putative conclusions regarding Bertrande in the *Arrest Memorable* because the wife was, of course, female.
[33] Davis, *Return of Martin Guerre*, 86.
[34] Davis, *Return of Martin Guerre*, 86, 91.

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GIVEN THE EVIDENCE REGARDING THE STORY OF MARTIN GUERRE, the assumption that Bertrande was Arnaud's accomplice necessarily involves convoluted reasoning and unsubstantiated assertions. As Coras perceived, Bertrande's accommodation to the charge of imposture and her timorous conduct in court argued that she was acting under duress, just as her refusal to swear that the defendant was not her true husband confirmed the court's suspicion that she had been suborned by her family. As far as one can tell, Bertrande's most fervent belief seems to have been in marital fidelity. At any rate, her sense of loyalty (rather than "shrewd realism") nicely explains her behavior throughout: her refusal to divorce, her "incredible longing . . . to have her husband back," her willingness to accept the proofs that Arnaud was her husband, her stand with him against family pressure, and her tearful submission to her true husband when she realized her error.[35] In the story of Martin Guerre, there is no need for unfounded hypotheses regarding maneuvering within sexual constraints, the perpetual uniqueness of sexual behavior, and secret pacts between a predator and his prey.

Having begun with such hypotheses, however, Davis is faced with the challenge of answering two resulting questions. First, how can one regard Bertrande as Arnaud's accomplice and yet still consider her as "honorable"? Second, what point of view was expressed in Coras's *Arrest Memorable*, Davis's principal source for her own interpretation, toward Bertrande and Arnaud?

Davis's determination to salvage "the honor of Bertrande de Rols"[36] is evident in the three contexts she establishes for regarding the relationship between the wife and the impostor. First, Bertrande and the pseudo-Martin fall in love, so their fraud fades in the glow of mutual affection. According to Davis, Bertrande had not merely prayed for Martin's return but had "dreamed of a husband and lover who would come back, and be different." She accepted the impostor because what she had "with the new Martin was her dream come true," a man she could live with "in passion."[37] Of course, the man she was living with continued to ferret out information from Bertrande about his presumed background. To Davis, that is not evidence of treachery and manipulation but rather a manifestation of the "intimate exchanges between husband and wife," which were "an ideal of Christian humanists and Protestant moralists," as well as an expression of "the Occitan delight in conversation" typically found in "the words of peasant lovers."[38] In describing Arnaud's challenge to the suborned Bertrande to swear he was not her husband, Davis places the impostor's maneuver in romantic guise: "He then made a test of her love and expressed his own."[39]

To be sure, Arnaud may have come to have affection for the trusting peasant woman, but, not surprisingly, the sources are silent on the matter. It is perhaps noteworthy that, while the impostor asked for Bertrande's pardon on the ladder

[35] The quotation is from Coras's description of Bertrande's reasons for accepting Arnaud as her husband: "Ioint l'incroyable envie, qu'icelle de Rolz avoit, de recouvrer son mary"; *Arrest Memorable*, 82.
[36] Davis, *Return of Martin Guerre*, 27.
[37] Davis, *Return of Martin Guerre*, 34, 44.
[38] Davis, *Return of Martin Guerre*, 46.
[39] Davis, *Return of Martin Guerre*, 69.

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up to the gibbet, he never tried to justify his duplicity by claiming that he had acted out of love for her. None of the evidence adduced by Davis, such as Bertrande protecting Arnaud from a beating or his trusting her to support him in court,[40] testifies to more than Bertrande's fidelity or to the impostor's exigencies. There is no basis for asserting that the historical record "everywhere attests to his having fallen in love with the wife for whom he had rehearsed and her having become deeply attached to the husband who had taken her by surprise."[41]

Second, Davis suggests that the loving couple probably discovered a religious justification for their deception in the Protestantism that was reaching into the Toulouse region, especially in a non-sacramental view of marriage that allowed these adulterers to conceive of "marriage as something that was in their hands to make, indeed, as in their hands alone." Davis is aware that this speculation has the disadvantage of rescuing Bertrande from the frying pan of adultery only to throw her into the fire of bigamy. Nevertheless, she presents the romantic spectacle of the pseudo-Martin and Bertrande finding solace and "another justification for their lives" in the Reformed religion: "That they could tell their story to God alone and need not communicate it to any human intermediary. That the life they had willfully fabricated was part of God's providence."[42]

The only evidence that Davis puts forth for these sentiments is that some Reformed soldiers and local converts to Protestantism smashed the altar of the Artigat church, although that was eight years after Arnaud's execution; that the de Rols family became Protestant, although Davis only cites documents regarding that family in the mid-seventeenth century; and that no priest played a major role in Arnaud's trial.[43] Davis suggests that it is also significant that Arnaud showed "respect" for his principal inquisitors, "men who were already attracted to Protestantism"—as if the charming impostor would dream of showing disrespect for judges who held his life in their hands, no matter what their religious views; or as if the judges would have been so given to fellowship with the accused peasant as to make him aware of their dangerous confessional leanings. Davis also finds it significant that Arnaud's final statement of contrition included no Catholic formulas or references to the saints.[44] Yet this negative consideration does not advance the argument that the putative accomplices sought justification for their illicit liaison in the new faith. All the sources say about Arnaud's spiritual sympathies is that, as a youth, Arnaud was notorious as "a constant denier and blasphemer of God's name," habitually swearing "on the head, body, blood, and wounds of our Lord."[45] This evidence much predates Arnaud's appearance in

[40] Davis, *Return of Martin Guerre*, 44–46.
[41] Davis, *Return of Martin Guerre*, 44.
[42] Davis, *Return of Martin Guerre*, 47, 48, 50.
[43] Davis, *Return of Martin Guerre*, 48–49; 142n.10.
[44] Davis, *Return of Martin Guerre*, 50.
[45] Arnaud was "consummé, en tous vices, adonné à toute espece de larrecins, et affrontemens, ordinaire renieur, et blasphemateur du nom de Dieu . . . Les tesmoings rapportoient, qu' iceluy du Tilh, estoit coustumier, iurer teste, corps, sang, et playes de nostre Seigneur, ce que vulgairement on appelle *Blasphemer*" (Coras, *Arrest Memorable*, 45; compare Davis, *Return of Martin Guerre*, 37). Although Davis argues that the publications of Coras and Le Sueur on the Martin Guerre case had a certain Protestant setting (107), she does not consider that the absence of Catholic formulas or references to

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Artigat, but at the very least it does not bespeak receptivity to the Reformed religion. In all, there is no warrant in the sources for introducing a religious dimension to the Martin Guerre story, not to mention the speculation that "local Protestant sympathizers [in Artigat] tended to believe the new Martin and the Catholics tended to believe Pierre Guerre."[46]

HAVING SUMMONED UP ROMANCE AND RELIGION TO ILLUMINATE the complicity of Bertrande and Arnaud, Davis also calls on notions of psychological reconstruction and reflectivity that likewise shift the focus of the story from deplorable deception to heroic commitment. From this perspective, the wife and the impostor were not being fraudulent and adulterous; rather, they were engaged in "self-fashioning," in "inventing" a marriage, in creating new "identities." Davis borrows the term "self-fashioning" from a recent work on Renaissance literature, and she takes it to refer to "the molding of speech, manners, gesture, and conversation" that aided advancement in elite circles.[47] Davis is concerned, however, to show that the fate of the elite and that of peasants was sometimes similar, that common folk too could fashion their lives in novel, self-conscious ways, and, further, "that an impostor's fabrication has links with more ordinary ways of creating personal identity." Unfortunately, Davis proves none of these things. Instead, after asserting that some sort of construction of identity may be seen in a person posing as another, she goes on to claim that, with Arnaud's preparations for his appearance in Artigat, "it is clear that . . . [the impostor] was moving beyond the mask of the carnival player and the stratagems of the mere inheritance seeker to forge a new identity and a new life for himself."[48]

There is nothing to justify such an elevated and exculpatory perspective of Arnaud du Tilh's imposture. Davis points to nothing in the historical record—no hint of self-reflection by the trickster, no provocative use of language in the sources, no revealing pattern of behavior, no suggestive inconsistencies or contradictions, no illuminating contrasts with other impostors—to substantiate her employment of "self-fashioning." Pervasive and tendentious, the concept is merely imposed on the historical record as an ingenious assertion, a modish way of viewing sixteenth-century peasants. Viewed through the lens of self-fashioning, Arnaud is an audacious forger of self, not simply a clever fraud, while Bertrande is an assertive molder of identity rather than an unfortunate dupe; together, the inventive rustics fabricated a marriage.

the saints may have been the result of the biases of the chroniclers rather than the supposed Protestantism of the impostor.

[46] Davis, *Return of Martin Guerre*, 56.

[47] Davis, *Return of Martin Guerre*, 103. In the work from which Davis borrows "self-fashioning," the term refers to considerably more than the personal prerequisites for courtly success: see Stephen Greenblatt, *Renaissance Self-Fashioning: From More to Shakespeare* (Chicago, 1980), 1–9. For some reflections on the story of Martin Guerre, see Stephen Greenblatt, "Psychoanalysis and Renaissance Culture," *Literary Theory/Renaissance Texts*, Patricia Parker and David Quint, eds. (Baltimore, Md., 1986), 210–24.

[48] Davis, *Return of Martin Guerre*, 4, 40–41.

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It is easy to cast the story of Martin Guerre into self-fashioning terms, but the exercise does not result in a plausible account of the desired "motivations and values."[49] According to Davis, when Arnaud asked Pierre Guerre for a financial accounting of the real husband's property—the action that eventually led to Arnaud's trials—"he was not just an impostor trying to take Martin Guerre's money and run" but rather was giving "a sign of how comfortable he felt in his role." Martin Guerre did not return to Artigat merely because he was alarmed at another taking his wife and land but may be regarded as having "come back to repossess his identity, his persona, before it was too late."[50] Nothing loath, Arnaud matched the long-lost husband persona for persona: "It would be a mistake to interpret his behavior [in court] . . . as simply a desperate attempt to stay alive. Live or dead, he was defending the identity he had fashioned for himself against a stranger." When Arnaud continued to deny his imposture after the return of Martin Guerre, his obstinacy did not reflect panic but rather how sincerely he had transformed himself into "the jealous husband."[51]

Davis takes the idea of the impostor refashioning himself as Martin Guerre very seriously indeed. In the central chapters of her book (chaps. 5–6), she scrupulously tries to avoid referring to the impostor as "Arnaud" or "Pansette," and she never adopts the chroniclers' appellations of the "Pseudo-Martinus" and the "soy disant Martin Guerre." For her, the refashioned rustic, "a more real Martin Guerre"[52] than the original, is generally "the new Martin." In dealing with the relationship between the supposed accomplices, Davis uses the impostor's name only once, when stating that "Pansette's rehearsals began once again," with the impostor and the wife reviewing the sexual episodes of the Guerres' marriage for information with which to dupe the court. Sometimes, when Davis uses the impostor's real names, it is to emphasize his supposed transformation, as when she refers to "the rebirth of Arnaud du Tilh as Martin Guerre." Sometimes, Arnaud is simply "Martin," the distinction between refashioner and real spouse collapsing as thoroughly for the modern historian as it allegedly did for Arnaud du Tilh.[53] This is unfortunate, for in examining the tale of the famous imposture it is surely essential to keep straight who was Bertrande's husband, what was a marriage, and who was really Martin Guerre.

The notion of self-fashioning functions as a way of elevating an interpretation of complicity between Bertrande and the pseudo-Martin from the sordid reality of fraud and adultery to that elusive realm where life approximates literature. Evoking imagination, intelligence, and subtlety, self-fashioning allows Davis to regard Bertrande and Arnaud as engaged in the same sort of artful construction of identity that supposedly typified princely courts and literary circles. A tale of

[49] Davis, *Return of Martin Guerre*, 4.
[50] Davis, *Return of Martin Guerre*, 60, 84.
[51] Davis, *Return of Martin Guerre*, 91.
[52] Davis, *Return of Martin Guerre*, 113.
[53] Davis, *Return of Martin Guerre*, 57, 50. In the central chapters, Davis uses "Pansette" six times; "Arnaud," ten; and "the new Martin," forty-five. "Martin" is used to identify the impostor six times (52, 54, 56, 57). Thus Davis refers to the impostor as "the new Martin" or "Martin" three-fourths of the time.

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mere trickery, as in the old version of the Martin Guerre story, piques curiosity but cannot sustain interest or analysis. A discourse on self-fashioning, on the other hand, is compelling to a modern sensibility preoccupied with ideas of role-playing and identity, beguiled by intricate theory and psychologizing. In effect, self-fashioning, along with romance and religion, glosses the conduct of the adulterous peasants. "The self-fashioning Bertrande" embraced "her dream come true" and invented a new marriage; the new, improved "Martin" found a new love, a new religion, and constructed a new self: "[T]he life he was fashioning for himself was operating like a conversion experience, wiping away the blasphemer . . . if not totally the trickster."[54] Of the trickster, there is abundant evidence, even a confession; for all the rest, there is no evidence whatsoever.

AFTER DEMONSTRATING HOW BERTRANDE THE ACCOMPLICE MAY BE REGARDED as "honorable" when seen in the light of romance, religion, and self-fashioning, Davis is faced with a second crucial question: how can her view of Bertrande and Arnaud be reconciled with Jean de Coras's, especially since the *Arrest Memorable* is Davis's principal source for her own very different interpretation? Her answer to this depends entirely on a complicated psychological reading of both Coras and his text. She sees Coras as embodying a profound ambivalence toward the self-fashioning peasants, while she regards the *Arrest Memorable* as "an historical account that raises doubts about its own truth," much as the judges of the Criminal Chamber supposedly displayed "mixed feelings" even as they condemned the dazzling trickster.[55] Davis asserts that Coras came to identify himself with Arnaud, probably a Protestant like himself, poised and eloquent like himself, with a beautiful wife like his own, and a liar so inventive that his fabrications were akin to "self-fashioning," a practice that lawyers and royal officers "knew all about."[56] Thus Davis neatly employs her unfounded hypotheses regarding Protestantism and self-fashioning to argue for an equally unfounded identification of the inquisitor with the accused.

Davis establishes another point of identification with her assertion that Coras was a kind of attenuated Pansette of the legal commentary in that "he lies a little" when shaping his account to strengthen it as "a moral tale." Yet she neither spells out the moral she perceives in Coras's text nor does she indicate any egregious shaping of evidence by the legal theorist.[57] Her list of "exaggerations and omissions" in the

[54] Davis, *Return of Martin Guerre*, 118, 49.

[55] Davis, *Return of Martin Guerre*, 108, 91n. According to Davis, the judges showed "lingering respect for the man who had dazzled them with his testimony" by treating his interests (that is, his property and his child by Bertrande) with consideration, by not torturing him, and by canceling his formal apology before the Criminal Chamber (89–91). But there is no evidence of "respect" shown by the court toward Arnaud. The judges were concerned for the interests of the Guerre family in dealing with the property and child; torture was not always used by the Toulouse court; and the apology was canceled when it seemed clear that Arnaud had no intention of apologizing, not (as Davis hints) because of fear that he would name Bertrande as his accomplice.

[56] Davis, *Return of Martin Guerre*, 102–03.

[57] Davis, *Return of Martin Guerre*, 108–09. Davis suggests that Coras may have seen "the story of Martin Guerre as conveying a Protestant message," but she concludes that "if Coras and Le Sueur had

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Arrest Memorable is not persuasive. Her accusation that the commentator in one instance neglected to report a failure of Arnaud's memory is trivial, and the contention that Coras "presents himself and the court as less convinced of Arnaud's innocence than in fact they were" is overstated.[58] The Criminal Chamber's view of Arnaud's probable innocence is expressed clearly in Coras's statement that only the intervention of God, as manifest by the nearly miraculous return of Martin Guerre, prevented "a horrible and monstrous imposture" from going unpunished.[59]

According to Davis, there is exaggeration in Coras's discussion of crimes such as abduction and sacrilege, of which Arnaud was not actually convicted. In her view, those annotations "gave him a chance to argue that Bertrande was coerced and that the death penalty was warranted."[60] Coras, however, did not need to grasp at opportunities to present Bertrande as a victim, as if he had to repress his recognition of the wife's outrageous behavior. The judge's annotations were intended to detail the manifold ways in which the unfortunate woman had been victimized, even if the culprit had not been charged by a court with the particular offense.[61]

Davis claims that Coras also exaggerates at another point, when "Arnaud du Tilh's prodigious qualities are built up by comparison with biblical, classical, and more recent impostors."[62] But Coras's concern clearly was to discuss precedents and parallels for the case, not to enshrine Arnaud in a pantheon of heroes. The lawyer no more intended to have his readers see Arnaud as "prodigious" as Jupiter and Caesar than he wanted them to regard Bertrande as illustrious as Cleopatra, Cato, Brutus, Hannibal, and Empedocles because, like them, she said she preferred death to dishonor. Indeed, Coras was aware that a reader might misconstrue his intentions, and, hence, after discussing Arnaud's memory in the context of feats of recollection by Cyrus, Seneca, and Caesar, he was careful to warn that he intended no equation "of such an impudent impostor with such noble, grand, and illustrious persons."[63]

All these supposed exaggerations and omissions, Davis claims, were "loopholes" in the *Arrest Memorable*, revealing Coras's uncertainties and allowing his commentary to be recast as a tragic drama.[64] She finds the most "curious omission" in the

such views, it must be said that they are not imposed by their texts"; 107. The reader, then, is left in some confusion as to the message or moral that Davis believes Coras intended his work to convey.

[58] Davis, *Return of Martin Guerre*, 109, 108.

[59] Without "le tout bon et puissant Dieu," "une si horrible, et monstrueuse imposture, demeurast celee, et incogneue"; Coras, *Arrest Memorable*, 70. Davis also maintains that Coras "never mentions that Bertrande and Pierre were imprisoned for months"; Davis, *Return of Martin Guerre*, 108. But Coras states "car elle demeuroit par l'appel encor arrestee"; *Arrest Memorable*, 84.

[60] Davis, *Return of Martin Guerre*, 109.

[61] For example, see Coras, *Arrest Memorable*, 98, on how Bertrande was a victim of rape, albeit by deception rather than violence.

[62] Davis, *Return of Martin Guerre*, 109.

[63] "Ce que i'entends avoir escript, avec la protestation, qu'ay cy devant faite, de ne vouloir entrer, en comparaison d'un si impudent affronteur, avec personnes si nobles, grandes, et illustres"; Coras, *Arrest Memorable*, 52. On the comparisons regarding Bertrande preferring death to dishonor, see *ibid.*, 84.

[64] Davis, *Return of Martin Guerre*, 110.

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first edition (1561) of the work to be an account of Arnaud's confession and execution, a flaw that leaves Coras's readers "some room for doubt about whether the Criminal Chamber actually did get the right man." On the contrary, no reader of Coras can doubt that the court and everyone else concerned had discovered the truth of the matter.[65] In any case, Coras made up for his "curious omission" in the second edition (1565) of the *Arrest Memorable*, where he devoted a passage to Arnaud's confession and execution. According to Davis, however, the judge could not help expressing his "conflicting feelings" about the dazzling criminal, since "ambiguity is reintroduced immediately" by a description of Arnaud's fate as a "tragedy," a comment that Davis chooses to see as expressing ambivalence about the impostor's guilt rather than dismay at his perverted talents.[66] This misconstruction is pivotal to Davis's argument, for she goes on to relate Coras's remark about tragedy to another writer's linking of the tragic with "prodigious" passion—"an association," Davis handily reminds the reader, "also suggested by Arnaud and Bertrande"—thereby proposing that Coras actually saw tragedy not in the self-destruction of a "fine peasant" but in the doomed romance of the wife and impostor. Again, self-fashioning crowns the pyramid of assumptions: "That Coras could conceive of 'a play of tragedy between persons of low estate' depended on his being able to identify himself somewhat with the rustic who had remade himself."[67]

Davis reconciles her interpretation of the Martin Guerre story with Coras's *Arrest Memorable* by asserting that, while the judge explicitly considered the wife innocent, an examination of his text, with its significant exaggerations, omissions, and comparisons, reveals another, unsettled view of Arnaud—and therefore of Bertrande as well. Supposedly troubled by psychological identification with Arnaud, Coras unconsciously allowed an element of ambiguity and tension into his commentary on the case, a "multivalent representation" that exposes his uncertainties.[68] On the one hand, Coras saw Arnaud as a treacherous deceiver, a virtually diabolical fraud and thief; on the other hand, he identified with the dazzling self-fashioner and fellow Protestant. Similarly, Coras regarded Bertrande as an innocent dupe, yet at some inarticulate level he also recognized that she had conspired with the impostor: this guilty Bertrande "is present in Coras's text, but is less prominent than the duped wife." Even though Coras had to admit explicitly that profound wrong had been committed by the ruthless impostor, he secretly could not deny to himself that there was something "profoundly right about the

[65] Davis, *Return of Martin Guerre*, 111; Coras, *Arrest Memorable*, 86: "Ainsi l'imposture dudit du Tilh, estant entierement descouverte, et le nouveau venu de tous uniquement receu, et recogneu, pour Martin Guerre, et le proces, par ce moyen du tout instruit, pour estre iugé diffinitivement, et iceluy veu"; see also Davis, *Return of Martin Guerre*, 85.

[66] Davis, *Return of Martin Guerre*, 103, 111.

[67] Davis, *Return of Martin Guerre*, 112. The phrase "a play of tragedy between persons of low estate" is not from Coras but is quoted from Matteo Bandello's *Histoire tragiques*, the work that linked the tragic with "prodigious" passion and that does not discuss the story of Martin Guerre (see Davis, *Return of Martin Guerre*, 153 n. 16). There is, of course, no indication that Coras conceived of "a play of tragedy" between Bertrande and Arnaud.

[68] Davis, *Return of Martin Guerre*, 110, 120.

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invented marriage of the new Martin and Bertrande de Rols."[69] Davis suggests that a psychological difficulty lay behind Coras's conflicted feelings and uncertainties. An elite member of a patriarchal society, the learned judge identified with the pseudo-husband, "the rustic who had remade himself," but could not face the psychic threat represented by Bertrande: "The possibility of an honorable woman disposing of her body as she pleases is much more disturbing than the self-fashioning of Pansette."[70]

NOTHING IS CITED FROM CORAS'S TEXT TO SUPPORT THESE CONTENTIONS. Given the nature of the argument throughout, that is entirely understandable. If the guilty but honorable Bertrande is "present in Coras's text," she cannot be there in the usual sense that makes quotation of, or reference to, the text possible. If Coras repressed his recognition of Bertrande's guilt because of alarm at her freedom from sexual constraints, proof of that must be sought in curious omissions and subtle exaggerations in the text regarding Arnaud du Tilh. If Coras believed that the unmasking of "the new Martin" was a tragedy, his troubling recognition of that cannot be encompassed by traditional forms of scholarly evidence. If "Bertrande does not seem a woman so easily fooled" in bed by a rogue like Arnaud, it is to be expected that her subsequent deceits would elude documentation. Indeed, if Bertrande had collaborated with the impostor, she must have been embarked on an enterprise so secret and subtle that it escaped the notice of all her contemporaries as well as "every scrap of paper left . . . by the past." Such arguments, it may be said, make footnotes to sources quite beside the point. If historical records can be bypassed so thoroughly in the service of an inventive blend of intuition and assertion, it is difficult to see what distinguishes the writing of history from that of fiction. As Montaigne observed about assertions being imposed on reality, "What can we not reason about at this rate?"[71]

Davis claims that the judges of Toulouse, in considering the "inventiveness" of Arnaud du Tilh, had to pose for themselves the question, "Where does self-fashioning stop and lying begin?"[72] But the terms she ascribes to the judges should pose for her readers a more pertinent question: In historical writing, where does reconstruction stop and invention begin? The virtues of *The Return of Martin Guerre* are clear: its eloquent portrait of peasant life, its sense of communal values and prejudices, its sympathy for those outside the elite, its emphasis on the central role of women, its evocative detail and supple prose. Unfortunately, none of the central points of the book—the knowing Bertrande, the devious court strategy, the tragic

[69] Davis, *Return of Martin Guerre*, 112, 103.

[70] Davis, *Return of Martin Guerre*, 112. Davis follows this statement with a quotation from a letter from Coras to his wife that details a "strange dream" (112). The letter comes from several years after Arnaud's trial (see Davis, 98), and Davis does not explain how it substantiates anything found in the *Arrest Memorable*.

[71] Davis, *Return of Martin Guerre*, ix; Montaigne, "Of Cripples," *Complete Essays of Montaigne*, 791. For some comments on history and fiction, see Carlo Ginzburg, "Prove e possibilità: In margine a *Il ritorno di Martin Guerre* di Natalie Zemon Davis," in N. Z. Davis, *Il Ritorno di Martin Guerre* (Turin, 1984), 131–54.

[72] Davis, *Return of Martin Guerre*, 103.

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romance, the Protestant justification, the self-fashioning peasants, the conflicted judge, the "multivalent" text—depend on the documentary record. As a result, a famous tale is given a reinterpretation that finally bears more resemblance to a historical romance than to an account "held tightly in check by the voices of the past."[73]

Jean de Coras wrote the *Arrest Memorable* to relate a true, fascinating story, not to give the villain of the piece "another chance" to argue his case or charm his audience.[74] The moral of Coras's account has nothing to do with a tragedy of romance and self-fashioning but with the deadly weakness of perception, with the ways in which deception can reach into the most intimate relations and shape human destinies. The judge's compassion was wholly reserved for an unimportant, luckless woman, and perhaps one of his main purposes in writing about the story of Martin Guerre was to enable others to understand how circumstances conspired with Bertrande's own desires to make her an ideal victim for a cunning impostor.[75] In the end, it cannot be said that Jean de Coras was successful in that ambition. The events of Artigat will probably never again be analyzed in detail, hence the historical image of Bertrande will be fixed forever in the unlikely pose established by Davis's reinterpretation. Humiliated by her husband's impotence, abandoned by him for years, duped and seduced by an impostor, harassed by kinfolk, and shamed before her community, Bertrande de Rols now suffers the posthumous fate of being refashioned into an assertive and principled champion, the shrewd and ardent companion of a man who transformed himself for her. No longer a dupe and victim, she has become a heroine, a sort of proto-feminist of peasant culture.

This Bertrande de Rols seems to be far more a product of invention than of historical reconstruction. The same may also be said of Arnaud du Tilh, the forger of identity, and of Jean de Coras, the ambivalent inquisitor. As interesting, subtle, and complex as these characters are, it is doubtful that they have anything to do

[73] After completing this essay, I read Janet Lewis's historical romance, *The Wife of Martin Guerre* (1941; rpt edn., Chicago, 1967). Davis states that Lewis's novel "differs from my historical account in most respects, but they resemble each other in presenting a Bertrande who is not a dupe and who has some independence of spirit" (Davis, 118n.). In Lewis's novel, however, Bertrande first suspects that the self-proclaimed Martin is an impostor after receiving him in bed (Lewis, 50). She sometimes thinks of him as "the new Martin" (Lewis, 50, 55). She slowly comes to realize "that she was consciously accepting as her husband a man whom she believed to be an impostor" (57; 55–65). Impelled by guilt and anger, she finally takes Arnaud to court, although even then she briefly hopes that the judges will declare him her husband (90). Arnaud's performance in court and his tenderness toward Bertrande make the wife ponder questions of identity (96–97). When the real husband returns, he accuses Bertrande of complicity with the impostor. Arnaud tells Bertrande that he had transformed himself into an honest man "for your beauty and grace" (107). He embraces his fate as a sacrifice to Bertrande, while the latter recognizes that "the return of Martin Guerre would in no measure compensate for the death of Arnaud" (108). Lewis draws an affecting portrait of a Bertrande torn between love and duty, a heroine at once shrewd, passionate, and honorable (55, 57, 63–64, 95–96). Romance and self-fashioning are prominent themes in Lewis's novel, along with the view that events unfold as a tragic drama for the peasant couple. Davis's version of the story mainly differs from Lewis's in her presentation of an explicit collaboration between the wife and the impostor, a religious perspective on that collusion, and a portrait of an unsympathetic Pierre Guerre.

[74] Davis, *Return of Martin Guerre*, 103.

[75] This is one of the dominant themes in the *Arrest Memorable*; see 10, 20–21, 33, 37, 54, 69, 80, 85, 98, 106, 109–12.

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with the actual story of Martin Guerre. The self-fashioning rustics and the conflicted scholar are nowhere to be found in the *Arrest Memorable*, a document that Davis implies is not to be trusted, in all its multivalent complexity, precisely because it fails to yield up the characters called for by her reinterpretation.

What Davis terms "invention," the employment of "perhapses" and "may-have-beens," is, of course, the stock in trade of historians, who are often driven to speculation by inadequate and perplexing evidence. Depth, humanity, and color in historical reconstruction are the products of imagination and do not flow from a vulgar reasoning upon data.[76] But speculation, whether founded on intuition or on concepts drawn from anthropology and literary criticism, is supposed to give way before the sovereignty of the sources, the tribunal of the documents. The historian should not make the people of the past say or do things that run counter to the most scrupulous respect for the sources. In discussing popular culture in preindustrial Europe, Davis has cogently observed that historians of that subject are strongly interested in people, "but I am not sure we really respect their ways very much; and this makes it hard for us to understand their lives."[77] Regrettably, in *The Return of Martin Guerre*, Davis has permitted an excess of invention to obscure the lives of the people who engaged her sympathy and imagination. If readers of her book feel a kinship with Bertrande and lament the return of the man with the wooden leg, if they feel that they truly understand the lives of those long-dead peasants, it is, all unbeknownst, at the expense of respecting their historical integrity, their very different motivations and values. It is one of the charms of Natalie Zemon Davis's generous and imaginative approach to historical study that she even leaves open that possibility in the epilogue of her book: "I think I have uncovered the true face of the past—or has Pansette done it once again?"[78]

[76] Compare Donald R. Kelley's review of Davis in *Renaissance Quarterly*, 37 (1984): 254.
[77] Natalie Zemon Davis, "Proverbial Wisdom and Popular Errors," in *Society and Culture in Early Modern France* (Stanford, Calif., 1975), 266.
[78] Davis, *Return of Martin Guerre*, 125.

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AHR Forum: The Return of Martin Guerre
"On the Lame"

NATALIE ZEMON DAVIS

THE STORY OF MARTIN GUERRE has been told many a time,[1] and flows so agreeably off tongue and pen that I cannot blame Robert Finlay for finding his own way to tell it again. I, too, welcome the chance to review once more its surprises and mysteries. As an afterthought, Finlay has noticed the last line of my *Return of Martin Guerre*, where I wonder whether my account could be wrong.[2] Indeed, my whole book, from its opening dedication to my husband, "my authentic husband," to its closing evocation of Pansette, the spirit of trickery, is an exploration of the problem of truth and doubt: of the difficulty in determining true identity in the sixteenth century and of the difficulty in the historian's quest for truth in the twentieth. "In historical writing, where does reconstruction stop and invention begin?" is precisely the question I hoped readers would ask and reflect on, the analogy with the uncertain boundary between self-fashioning and lying built into my narrative.

Although I would have preferred a critique more attentive to my text and to the *Arrest Memorable* of Jean de Coras, more subject to "the sovereignty of sources" to which it claims loyalty, I welcome Robert Finlay's response to my challenge. His essay raises important questions about historical method and interpretation, about the range of evidence that can be brought to bear on a historical problem, about

I am grateful to Anthony Grafton, Lisa Jardine, and Donald R. Kelley for their critical reading of and comments on this essay.

[1] Indeed, more times than I knew when publishing the first American edition of *The Return of Martin Guerre*. I use this opportunity to list additional accounts before 1900: [Jean de Coras], *Processo, et Arresto ò sentenza data dal Parlamento di Tolosa sopra d'un fatto prodigioso et memorabile, tradotto di lingua francese nella favella toscana, per Mag. Gio. Batt^a Forteguerri Dott^re Pistorese, con cento annotationi ornate et aggiunte da lui* (Manuscript, dedicated by Forteguèrri to the Grand-Duchess of Tuscany, April 1591); *Waerachtighe History, Van een Wonderbaerlick bedroch* (Leyden, 1616), based on Coras; "The Case of Martin Guerre," a four-part serial in the *New London Gazette*, 4–8 (9 December 1763–6 January 1764), drawn from Coras, the first American telling of the case; Gottfried Wilhelm Leibniz, *Nouveaux essais sur l'entendement humain* (1765), Book 3, chap. 3, "Termes généraux": paragraph 7 refers to "le faux Martin Guerre" and his successful imposture as part of his discussion of the idea of an individual; A. Fouquier, *Causes célèbres de tous les peuples* (Paris, 1865–67), 7: cahier 33, 1–10: "Le Faux Martin Guerre (1560)," based on Coras and Estienne Pasquier, with discussion about what Bertrande knew. I am grateful to Craig Harline, Philip A. Hamburger, and Vanessa Schwartz for passing on some of these references to me.

[2] Natalie Zemon Davis, *The Return of Martin Guerre* (Cambridge, Mass., 1983), 125.

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the way to read texts, and about the role of speculation and uncertainty in historical knowledge. He also offers his own theory of the Martin Guerre case, different from mine, different from that of Coras, and different from several others proposed since the publication of *The Return of Martin Guerre*. They make for interesting comparison, showing the complex possibilities in Coras's "prodigious history" and the interplay of scholarly approach and twentieth-century assumption in our historical work.

To frame my essay, I introduce some of the differences in approach between Finlay and me. Both of us would like to know "what happened" when Arnaud du Tilh arrived in the village of Artigat and stayed there as Martin Guerre for three years and more. I conclude that the reaction was a mix of being deceived and being deceptive or acquiescent, even in the village at large ("whatever doubts people had, they silenced or even buried them for a while,"[3]) and that especially Bertrande became Arnaud's collaborator, though toward the end not always a wholehearted one. Finlay concludes that the entire Guerre family and all the neighbors were fully tricked from the beginning and that Bertrande remained a dupe until she saw Martin Guerre in court and realized her error.

I have the additional goal, at least as important, of embedding this story in the values and habits of sixteenth-century French village life and law, to use them to help understand central elements in the story and to use the story to comment back on them—that is, to turn a legend into history. Finlay gives short shrift to my material on Basque customs, migration, property and land sales, inheritance, women's work, judicial practice, and the rest as "context" and "color in historical reconstruction." Or else, when it impinges too closely on his own moral concerns, as with my discussion of religion and marriage law as elements in the mental world of Arnaud and Bertrande, he dismisses it as "exculpatory." In contrast, he is content to leave a cultural and social void around this twosome (quite different from his practice in his valuable study of fifteenth-century Venetian patricians). He presents a timeless pair of "predator" and "prey," of "clever fraud" and "unfortunate dupe," as in an early modern legend or morality tale.[4]

Next, I think it important to explore cultural exchange across class lines, as when peasants and judges meet each other in a courtroom and when a village tale becomes a fascinating subject for learned literature. I find helpful clues here in the perceptions of likeness, difference, and danger in the life story of the "other" and in the appearance of key words and concepts (such as "tragedy") in startling and unusual places. Texts also provide me fresh evidence in their overall organization and in the rules for their creation: literary and narrative structure are part of the "data" upon which I want to do "vulgar reasoning" to get at a sixteenth-century argument.[5] Finlay is interested in cross-class relations in the case of fifteenth-century political exchange between the Venetian patriciate and the *popolo*, but he

[3] Davis, *Return of Martin Guerre*, 43.

[4] Robert Finlay, *Politics in Renaissance Venice* (New Brunswick, N.J., 1980). Robert Finlay, "The Refashioning of Martin Guerre," *AHR*, 93 (June 1988): 571, 562, 564.

[5] Finlay, "Refashioning of Martin Guerre," 571.

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does not recognize socio-cultural exchange as a serious historical event.[6] Speculating on psychological reactions to others, even when the life is well documented and the reactions closely linked to sixteenth-century concerns, seems to Finlay unreasonable, although he feels free to rely on, say, the "exaggerated [self-] importance," "self-righteous[ness]," "bitterness," or "impulsiveness" of his Venetian subjects.[7] He is indifferent to the manifold structure of the *Arrest Memorable*, to the meaning of its language, and to the place of his few citations in the course of Coras's argument. Finlay will extend literary concern to the diary of Marino Sanuto but not to a text as strange as the *Histoire prodigieuse*.[8]

Finally, there is a contrast in our mental habits, cognitive styles, and moral tone. I see complexities and ambivalences everywhere; I am willing to settle, until I can get something better, for conjectural knowledge and possible truth; I make ethical judgments as an assay of pros and cons, of daily living and heroic idealism. Robert Finlay sees things in clean, simple lines; he wants absolute truth, established with no ambiguity by literal and explicit words; he makes moral judgments in terms of sharp rights and wrongs. I can conceive of a peasant woman "honorable" in her own eyes and those of her women neighbors by sixteenth-century standards who decides to accept a false husband, lacking any other, and who could be understood and pardoned by those neighbors. Finlay finds this a contradiction in terms and asserts, "There is no doubt that if Bertrande's contemporaries believed she was guilty, she would have been seen as despicable as the impostor."[9] If Finlay's book on Venetian government is much less categorical in tone, in "The Refashioning of Martin Guerre," he plays Savonarola to my Montaigne. Fortunately, historical practice can profit from each stance.

First, let me spell out for readers my research method. My core sources were the *Arrest Memorable* of Judge Jean de Coras, the special commissioner and reporter for the Guerre case to the Parlement of Toulouse; the *Historia Admiranda* (*Histoire Admirable*)[10] of Guillaume Le Sueur, a text I discovered and whose author turned

[6] Finlay, *Politics*, 44–59.

[7] Finlay, *Politics*, 12, 256, 278, 235.

[8] Finlay, *Politics*, 10–12, 276–77. The title of the first edition of Jean de Coras's book is *Arrest Memorable du Parlement de Tolose, Contenant une histoire prodigieuse, de nostre temps, avec cent belles, & doctes Annotations* (Lyon: Antoine Vincent, 1561). The edition I am using is one of the printings of the 2d edn.: *Arrest Memorable du Parlement de Tholose, Contenant Une Histoire prodigieuse d'un supposé mary, advenüe de nostre temps: enrichie de cent et onze belles et doctes annotations* (Paris: Galliot du Pré, 1572).

[9] Finlay, "Refashioning of Martin Guerre," 556. Similarly, I am willing to consider a letter written by Coras to his wife seven years after the trial (lacking one closer) in which he recounts a nightmare, "the severity and threat of a strange dream," about her turning from him and remarrying another before his eyes, as an indication of his feeling about a "beautiful" peasant woman, attested to as "honorable," doing a similar thing (Davis, *Return of Martin Guerre*, 112–13, 20). Finlay objects to using a letter from a later date and can see no relevance of the letter to events in the Martin Guerre trial (Finlay, "Refashioning of Martin Guerre," 569 n. 70). Of course, he is demanding "substantiation"; I am suggesting evidence that can be brought to bear on Coras's frame of mind and his representation of Bertrande in the *Arrest Memorable*.

[10] [Guillaume Le Sueur], *Admiranda historia de Pseudo Martino Tholosae Damnato Idib. Septemb. Anno Domini MDLX* (Lyon: Jean de Tournes, 1561); *Histoire Admirable d'un Faux et Supposé Mary, advenue en Languedoc, l'an mil cinq cens soixante* (Paris: Vincent Sertenas, 1561).

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out to be a law clerk for another judge in the trial (a text not seen by Finlay and mentioned by him only in a footnote); and the decisions concerning the case in the registers of the Parlement of Toulouse. Around these, I clustered evidence of peasant doings and sayings from all the village archives in question (the Artigat of the Guerres and the Rolses, the Basque country of the Daguerres, Arnaud's Sajas, and their environs); from the criminal records at Toulouse; and from sixteenth-century printed accounts about the region. In the absence of the full depositions and testimony from the trials and of rural diaries and letters, this is the best one can do in the study of a primarily illiterate, sixteenth-century peasant society. The world of the judges and their court I built from the same concentric circles of research, starting with evidence from the jurists themselves—books and letters they wrote, cases they judged, and the like—and spreading to the practice of other criminal courts and the prescriptions for criminal procedure published by sixteenth-century judges. Throughout, I worked as a detective, assessing my sources and the rules for their composition, putting together clues from many places, establishing a conjectural argument that made the best sense, the most plausible sense, of sixteenth-century evidence.[11]

In addition, I decided to use a literary construction for *The Return of Martin Guerre* that would allow the book to be read, if one wished, like a detective story (or like Coras's *Arrest Memorable*) at a single sitting. I also chose to advance my arguments—about the case, about rural society, about identity, about doubt—as much by the ordering of narrative, choice of detail, literary voice, and metaphor as by topical analysis. This was partly because I wanted the book to be accessible to a varied audience, from the villagers of Artigat whom I had interviewed to specialists in sixteenth-century rural, legal, and literary history, who I hoped would enjoy my new findings. Even more important, I wanted to develop an expository style for the first part of the book that could provide the equivalent of cinematic movement, with flash-forwards rather than flashbacks.

This rhetorical and narrative strategy clearly has its costs, especially for the scholar's leisurely back-and-forth between source and subject. To be sure, the chapters on the trials at Rieux and Toulouse give much information on the pros and cons of judicial deliberation, and the last three chapters are devoted to the literary production and content of the texts by Coras, Le Sueur, and others. But the research evidence for the story line of the imposture is packed into the notes, available for the patient researcher, but with only an occasional one giving the prose discussions and explicit weighings needed to assist those unfamiliar with legal or archival material.[12] In addition, readers, always an independent lot, may miss irony, ignore metaphors and signposts, and even be lost without an argument that begins "there are four reasons." On the other hand, it has seemed to me that the strategy of *The Return of Martin Guerre* could have distinctive fruits: the

[11] On historical knowledge as conjectural knowledge and historians as detectives, see R. G. Collingwood's celebrated "Epilegomena" to *The Idea of History* (New York, 1956), especially 231–302; Carlo Ginzburg, "Morelli, Freud and Sherlock Holmes: Clues and Scientific Method," *History Workshop*, 9 (1980): 5–36; and Robin Winks, ed., *The Historian as Detective: Essays on Evidence* (New York, 1969).

[12] For example, Davis, *Return of Martin Guerre*, 143 nn. 6, 11.

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re-creation of complexity in historical experience (here, in the case of villagers), the recapture of the interplay between the socially determined and the chosen, and the encouragement of readers to think about the implications of literary construction for historical representation.[13]

WE WILL BEGIN WITH BERTRANDE DE ROLS and what she "knew" about Arnaud du Tilh when they lived together as husband and wife. I put quotation marks around "knew" to evoke my phrase "by explicit or tacit agreement, she helped him become her husband" and the suggestions I make about how Bertrande—and, indeed, her relatives and her fellow villagers—could slip into a relationship with an unknown person, quieting doubts, privileging signs of identity, and ignoring contrary clues because a return of a missing husband was so much to be desired.[14] As an abandoned wife, Bertrande had the most at stake, but she also had early and the most intimate access to the contrary clues. The degree of her conscious accommodation to them is hidden from us: one could imagine indirection at the start, not only with the man whom she allowed to be Martin Guerre but also with herself. But somewhere along the way—early or late—their collaboration had to have been openly planned.

The evidence for Bertrande's collusion with Arnaud du Tilh rests not on one argument, as Finlay alleges, but on four, all drawn from the legal sources: the fact, affirmed by all witnesses, that Arnaud greeted everyone by name and knew something of their past when he first saw them in Artigat; the fact, affirmed by many witnesses and confirmed by Jean de Coras, that Arnaud was of quite *different body size from Martin Guerre*; *the character of* Bertrande's behavior from the time that Arnaud was accused of being an impostor by Pierre Guerre and others until the time she became a plaintiff against him; and the character of Bertrande's testimony during the trial at Toulouse, which allowed Arnaud to persuade the court he was Martin. The implications for Bertrande's knowledge and intentions are stronger for certain elements than for others, but together they make a good and very plausible case for collaboration.

The law clerk Le Sueur gives us a description of Bertrande's first meeting with Arnaud as it was described in the trial. Arnaud had set himself up to rest at an inn in a nearby village. When Bertrande first saw him there, "she was uneasy for a long time, expressing doubt and turning away from him."[15] Only after he had spoken to her in an affectionate voice, recalling what they had said to each other the first night of their marriage and reminding her of the white hosen he had left in a coffer

[13] On the implications of different rhetorical styles in historical writing, see Allan Megill and Donald N. McCloskey, "The Rhetoric of History," in John S. Nelson, Allan Megill, and Donald N. McCloskey, *The Rhetoric of the Human Sciences: Language and Argument in Scholarship and Public Affairs* (Madison, Wis., 1987), 221–38.

[14] Davis, *Return of Martin Guerre*, 43–44.

[15] "Eum illa primùm contuita, diu anxia restitit: et dubitanter admittens, iam aversari velle videbatur" (*Historia*, 6). "Quand elle fut arrivée devers luy, de prime abordée elle se retint comme esbahie et ne vouloit aprocher; mais comme doubteuse, se retiroit en arrière" (*Histoire*, Bi[r]). Coras does not give details of this first encounter except to mention the white hosen: "Et de premiere rencontre luy dit, Va-moy querir les chausses blanches, doubles de taffetas blanc, que ie laissay dans un tel coffre quand ie parti" (*Arrest Memorable*, 63).

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the day of his departure, did she embrace him and say that his growth of beard had made it difficult for her to recognize him. When Pierre Guerre arrived, he had the same initial reaction and did not accept Arnaud as his nephew Martin until he began to talk to him of business arrangements they had had before he left.[16] Thus Bertrande and Pierre said they doubted at first that they were seeing Martin Guerre.

Bertrande and Arnaud did not go directly to Artigat—continuing with Le Sueur's account of what was said at the trial—but stayed at the inn for some days while she tended him for an illness. He told her it was the pox, and he did not ask her to sleep with him right away. When he was better, she took him back to her village, where, as Coras reports following the testimony "of almost all the witnesses,"

> he greeted by name almost everyone he met who knew Martin Guerre, without having otherwise seen them or known them; and if they found it difficult to recognize him, he brought to mind all kinds of things from the past, and said to each one in particular, "Don't you remember when we were at such and such a place, ten, twelve, fifteen or twenty years ago, and we did such and such a thing in the presence of so and so and where we talked about such and such?"[17]

This was one of the feats that led Coras to write that Arnaud must have had a familiar, must have raised a spirit through necromancy. How did he know all this, especially when it turned out that he and Martin had never met until the latter turned up at the court of Toulouse? Before he was executed, Arnaud named two friends of Martin who had given him information and described other inquiries he had made before his arrival in Artigat, *but Coras did not see how prior instruction could lead to such face-to-face encounters*, even for a man with Pansette's prodigious memory.[18] Unless Robert Finlay wishes us, too, to have recourse to a theory of magic, we have to imagine sustained assistance from a person or persons within the family—the sisters, Pierre Guerre, who after his initial recoil was going about talking to villagers of "his nephew," and most centrally Bertrande, who brought him back to Artigat. This assistance could have been forthright; it could well have been tacit, only a few turns beyond the ordinary mutuality of wife and returned spouse. To imagine a hypothetical case: "You remember Catherine Boëri, Martin—she brought us the drink on our wedding night."[19]

My second "sign" is Arnaud du Tilh's body and its touch to Bertrande. Finlay interprets my text to be arguing narrowly for a distinctive and clearly recognizable

[16] *Historia*, 6–7; *Histoire*, Bi^{v}.

[17] Coras, *Arrest Memorable*, 63.

[18] Coras, *Arrest Memorable*, Annotation 66 (*sic* for Annotation 71), 90: "Il y avoit certes grande raison de penser que ce prevenu eust quelque esprit familier, veu qu'il sçavoit si bien et veritablement respondre de toutes choses . . . Et (qui est bien plus à admirer) cognoissoit tous ceux qui se presenterent à luy du commencement, et apres sans les avoir veux oncques. Ce que ne pouvoit tomber en instructions, ni memoires qui luy eussent esté baillées par autre." And again: "On peut enseigner certains propos, donner des enseignes, et marques: mais de bailler la congoissance de tant et tant de diverses personnes, non iamais veuës ni cognues: cela est impossible, autrement que par Magie" (65).

[19] Coras, *Arrest Memorable*, 62. Boëri is a hypothetical case, as the actual testimonies from individuals Arnaud had greeted are all lost. At the trial, Catherine Boëri was a strong supporter of the accused as truly Martin Guerre (61).

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lifetime manner of sexual intercourse. Individuals may indeed have some long-term patterns of sexual behavior and expression, like a sexual fingerprint, but, of course, there is sexual learning (here, Robert Finlay and I agree): Martin and Bertrande learned to have sexual intercourse with each other by the last year or two before Martin departed, and one assumes that there were new gestures and movements in their married congress after he returned from Spain in 1560. A new kind of kiss from Arnaud du Tilh need not of itself constitute a contrary clue for Bertrande de Rols.

But the "touch of the man" is a broader idea. Let us remember that the resemblance between Arnaud and Martin was solely a matter of face and lineaments—not perfect, for some testified that Martin was darker than Arnaud and had a pug nose, but good. Testimony about scars, warts, extra teeth, and a blood mark in the eye was so diverse and divided that Coras said it did not add up to proof one way or the other. (Curiously enough, there was no testimony reported about voice or accent, even though Arnaud, with his Gascon background, would presumably have spoken langue d'oc differently from Martin with his Basque mother tongue, and the timbre of their voices might have varied as well.) Where there was accord was on the different body size and height of the two men: "Almost all these witnesses agreed that Martin Guerre was taller and darker, a man thin in body and legs . . . while the prisoner is short, stocky and well-furnished in body, and with thick legs." The village shoemaker also testified that Martin Guerre's shoe size was larger than that of the man claiming to be the husband of Bertrande de Rols.[20]

Thus, when Bertrande finally found herself in the embrace of Arnaud du Tilh, she was feeling a body quite unlike the one she had lain next to and held for nine or ten years, unclothed as well as clothed (early woodcuts of couples in the marriage bed show them with nightcaps but naked).[21] One possible reaction to that body is proposed by Coras in trying to understand why so many people mistook Arnaud for Martin: they would assume that he had filled out as he grew older.[22] The other possible reaction—I think more plausible for a wife, since Arnaud was not just stockier but shorter and squatter than her husband and she washed those smaller feet[23]—is that Bertrande "knew," despite the passage of years, that she was not being held by the real Martin Guerre.

At any rate, this is what Martin Guerre accused her of in court, when he returned on his wooden leg. The phrase "there is no mistaking 'the touch of the man on the woman,'" which Robert Finlay calls a proverb—and it does have a nice ring—is

[20] Coras, *Arrest Memorable*, 52: "Tous ces tesmoins presqu'accordent que Martin Guerre estoit plus haut et plus noir, homme gresle de corps et des jambes . . . où toutesfois le prisonnier est petit, trappe, et fourni de corps, ayant la iambe grosse." Coras repeats the contrast later: few of the marks reported on Martin Guerre were not found on the accused "except his height and breadth" ("reservé de la longueur, et grosseur") (74). On the shoemaker, see Coras, 53.

[21] Jean d'Arras, *Mélusine par Jehan d'Arras, Nouvelle édition conforme à celle de 1478* (Paris, 1854), 30^{r}, the bishop blesses the marriage bed of Raimondin and Mélusine, who have headdresses but no clothes on. As for smelly bodies, if this were the case, it would only increase the possibility of Bertrande's distinguishing between Arnaud and Martin and therefore add to the contrary clues.

[22] Coras, *Arrest Memorable*, 74.

[23] Coras, *Arrest Memorable*, 68.

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actually taken from *Les Recherches de la France* of jurist Estienne Pasquier, which I cited in the appropriate note.[24] Pasquier, who was a young Paris lawyer in 1560 and who had read both Le Sueur and Coras, describes what Martin said before the court after Bertrande had "asked his pardon for the wrong she had done him unknowingly": "How is it possible that you have given consent to this abuse? For my uncle and my sisters there is some excuse: but none in the touch of the man on the woman." Interestingly, Pasquier thought Martin Guerre deserved a punishment as serious as Arnaud's because "by his absence he had been cause of the wrongdoing."[25]

And let us hear Guillaume Le Sueur quoting the angry husband as he rebukes Bertrande for more than Finlay's "imprudence":

> How is it possible that a wife can be abused in this fashion without her knowledge or consent? An uncle and sisters can have some excuse, but the husband must be known to the wife better than to any other relative, for there is no tie closer than marriage. And that for this cause he could not pardon such a fault, and he had to presume that she had done it by desire and impure volition.[26]

Coras's quotation is more condensed:

> And don't excuse yourself by my sisters, nor by my uncle: for there is neither father, mother, uncle, sister or brother who must better know their son, nephew or brother than a wife must know her husband. And for the disaster that has befallen our house, no one has the wrong but you.[27]

In his discussion of that "disaster" ("malheur et desastre") elsewhere in his text, Coras uses citations that refer to a wife's adultery.[28]

My third "sign" is Bertrande de Rols's behavior from the time Arnaud was charged by Pierre with imposture until the day she became the plaintiff in the case against him before the judge of Rieux. When someone said he was not her husband, "she rudely insisted that that was a lie, that he was Martin Guerre her husband or else a devil in his skin, that she knew him well, and that if anyone

[24] Davis, *Return of Martin Guerre*, 141 n. 3.

[25] "Comment est-il possible (luy dit-il) que tu ayes presté consentment à cest abbus? Car et en mon oncle, et en mes soeurs il y peut avoir quelque excuse: Mais nulle en l'attouchement de l'homme à la femme." Estienne Pasquier, *Les Recherches de la France* (Paris, 1643), 571–72. I cited the 1621 edition in *The Return of Martin Guerre*.

[26] Le Sueur, *Histoire*, Diii^{r-v}: "Comme est il possible qu'une femme soit abusee en ceste façon sans son sceu et consentement? L'oncle et les soeurs en cela pouvoient avoir quelque excuse: mais le mary doit plus estre cogneu à la femme qu'à nul autre parent, et qu'il n'avoit parentage, ny lien plus estroict que le mariage: que pour ceste cause ne luy pouvoit pardonner une telle faulte: et qu'il estoit à presumer que elle l'avoit faicte par desir et volunté impudique." *Historia*, 16: "Etiam ne, inquit, deludi coniux, nisi volens potuit? Posse ignosci patruo, posse etiam sororibus. At uxori maritum debere esse notiorem, quàm ulle ex parentibus, aut sibi. Nullam esse cognationem, quae proprius nos contingat, nullum vinculum quo arctiùs colligemur. Proinde se ei errori, quem obtendat condonare non posse: qui diffluentis et ad turpitudinem abiectae suspicione non vacaret." These page numbers to these texts were fully cited in the appropriate endnote of my book, 148 n. 8.

[27] Coras, *Arrest Memorable*, 107: "Et ne vous excusez en mes seurs, ni en mon Oncle: car il n'y a pere, mere, Oncle, seurs ni freres, qui doyvent mieux cognoistre leur fils, nepveu, ou frere que la femme doit cognoistre le mari. Et du desastre qui est avenu à nostre maison, nul a le tort que vous."

[28] Coras, *Arrest Memorable*, 5, Annotation 2; and 140, Annotation 98. This citation by Coras is discussed below, p. 593.

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henceforth was so foolish as to say the contrary, she would make him die."[29] Martin's sisters supported her view, but others joined Pierre's accusation: her mother, married to Pierre Guerre; Pierre's sons-in-law; other villagers. Coras says it is understandable that a person persevere in an error if nothing happens to contradict it.[30] But there were public contradictions: the shoemaker told of differing feet size, others spoke of a strange loss of ability to fence and do acrobatics, the soldier from the Picardy wars denounced the man calling himself Martin Guerre, because the real husband had lost his leg at the battle of Saint Quentin. Bertrande maintained her stance, placing herself between Arnaud's body and the blows of Pierre Guerre and his sons-in-law and supporting him as her husband in a 1559 arson trial in Toulouse, when the accusation of imposture was first made before a court.

Now, let us hear how Coras reflects on whether Bertrande could be guilty in light of these circumstances:

> For during those three years, she often heard people murmur and several of them gave her arguments: specifically, that this personage was not Martin Guerre, against whom (although she had good reason to believe them or at least to wonder about what they said) she nonetheless vigorously fought, maintaining the contrary. In this behavior, one seems manifestly to uncover her guilt, for just as a possessor is said to be in good faith until he knows or suspects that the item he has acquired is from someone other than the person who turned it over to him, so a woman who sleeps with someone other than her husband is excused while she is ignorant and thinks she is with her husband; but when she comes to know the contrary or even to have doubts about it and nonetheless participates with him and allows herself to be known by him, she is inexcusable. Once she perceives or suspects the fraud, she must immediately separate herself from him.[31]

Bertrande showed no such circumspection. The night Arnaud returned from the trial in Toulouse, she washed his feet and slept with him as her husband. It was the next day, after he had been arrested at dawn, that Bertrande finally agreed to be plaintiff in a case of imposture already started falsely in her name by Pierre Guerre. The next day, "*without any new proof*" ("sans nouvelles preuves," emphasis mine), as Coras puts it, she changed her story.[32] That is, during the many months when public evidence was accumulating that the man in her bed was not Martin Guerre, she maintained stoutly that he was; within the course of a day, with no new evidence forthcoming, she stated to the judge of Rieux that he was not. Rather than interpreting Bertrande as so duped by Arnaud du Tilh that she naively ignored all contrary clues, it seems more plausible to assume that she had decided that he was a good enough husband ("Martin Guerre or a devil in his skin"), one worth defending in that role as long as she could.

[29] Coras, *Arrest Memorable*, 66–67.

[30] Coras, *Arrest Memorable*, 86, Annotation 67: "On ne presume point volontiers, et sans cause une erreur en personne quelconque, toutesfois depuis un homme est en quelque erreur, il est à presumer q'il persuere [*sic*] en iceluy s'il n'appert du contraire, car le changement de volonté n'est à presumer, et moins aussi qu'aucuns nouveaux accidens surviennent, qui le puissent causer."

[31] Coras, *Arrest Memorable*, 138, Annotation 98.

[32] Coras, *Arrest Memorable*, 67, Annotation 45.

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On that January day in 1560, what had changed were her circumstances. Pierre and his sons-in-law had bundled Arnaud off to prison, and, although Bertrande might hope for support from her sisters-in-law, her male protector was gone, a court case drawn up against him as the impostor Arnaud du Tilh. Pierre Guerre, patriarch of both the Guerre and the Rols families, and his wife—Bertrande's mother—were "threatening to throw her out of the house, if she did not accuse the prisoner."[33] Beyond this threat to her access to her children, to her relatives, and to family property was the threat to her reputation and even to her life if Pierre and her mother went so far as to accuse her of complicity in a serious case of adultery and fraud.[34] Other questions may also have troubled Bertrande (what about the Martin Guerre with the wooden leg?) and other feelings (guilt, a disquiet about and inner distancing from the man she had taken in as her husband—what if he tricked her again?). I think we have enough sense of her predicament to understand why she agreed to the court case.

But that was not the only action she took on that fateful day: she also sent clothing and money to the prisoner in Rieux, just as she had when he was a prisoner in the Seneschal's prison at Toulouse.[35] Is this an indication that she *still* thought the prisoner was Martin Guerre, as Robert Finlay would claim, or is it rather an indication of the partial support she would offer him in the months ahead?

My fourth and, in some ways, my strongest "sign" of Bertrande's collaboration with Arnaud du Tilh is her testimony during the trial at Rieux and before the Parlement of Toulouse. This is what I call her "double game" (I do not use the term, as Finlay thinks, to refer to Bertrande's acceptance of Arnaud as Martin, for I see all the Guerres as collaborators to some extent, all of them playing a similar game to begin with). At the trials, on the one hand, she satisfied Pierre Guerre and her own individual interests by insisting that she had been tricked and deceived for more than three years by Arnaud du Tilh, although, as Coras and Le Sueur both noted, she spoke in a hesitant and frightened way and refused to take an oath that he was not her husband. On the other hand, in separate interrogations, she always told stories about her intimate life with Martin Guerre that he could repeat perfectly in separate questioning, and she always confirmed the secret details of their early life together that he had provided in his separate testimony.

This convergence in testimony was a critical element in the Criminal Chamber's moving to a decision "more disposed to the advantage of the prisoner and against the said Pierre Guerre and de Rols."[36] Coras reviews at length the evidence collected by the lower court at Rieux and much increased by the hearings at Toulouse. The material evidence was inconclusive, both resemblances and differences being reported between the prisoner and Martin Guerre. Trustworthy relatives on either side took opposing views. Bertrande was especially baffling, a

[33] Coras, *Arrest Memorable*, 67–68.

[34] As I show from the parlementary sentences of the court at Toulouse, a wife was given a death sentence for adultery only in those few cases of very serious offense against social order—as when a nobleman's wife slept with her sharecropper (Davis, *Return of Martin Guerre*, 88, 148 n. 11).

[35] Coras, *Arrest Memorable*, 70, 68. Le Sueur, *Histoire*, Biii[r].

[36] Coras, *Arrest Memorable*, 87, Annotation 68.

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woman of "virtuous and honorable" reputation, who said she had been tricked for three years and more, and yet "in so long an interval, it is not plausible ['vraisemblable'] that she would not have recognized him for a stranger if the prisoner were not truly Martin Guerre."[37] That she was being suborned so to testify by the self-interested Pierre Guerre became more and more likely because of the prisoner's seeming to have an attribute that a sixteenth-century judge would take very seriously: the memory of Martin Guerre.

This memory was established not only by Arnaud's testifying to many facts about Martin's family, wedding, and activities before he left—facts that he could have obtained from his pre-Artigat informers, from Pierre Guerre in the early days, and from the loyal sisters, as well as from Bertrande—but also by his "pouring out," in Le Sueur's phrase, "those secret things that only a husband can observe." Bertrande had introduced just such detail to the court in justifying her initial acceptance of Arnaud as her husband. Among his "signs" to her, she said in the first trial, were "even the acts and words which are exchanged the most secretly between husband and wife, that others cannot rightly know or hear, up to telling her the places, times and hours of the secret acts of marriage . . . and their words, before, after and during the act." These are the matters that she would testify about to the judge at Rieux and especially to Coras and Judge François de Ferrières at Toulouse: details of the wedding night, of the ending of their period of "being tied" (that is, of being unable to have intercourse), of subsequent sexual encounters. Arnaud could always repeat them, and the ones he offered, she confirmed.[38]

I do not see how one can account for this convergence of testimony between Bertrande and Arnaud without a prior agreement between the two. Unless Robert Finlay wants to fall back with Coras on magic. Are we to imagine one of the judges or a scribe feeding plaintiff and defendant each other's words? Moreover, Bertrande could have at any time introduced a "secret act," remembered or fabricated, between her and Martin that she knew Arnaud could not repeat, and it would have provided evidence for the court that she was not being suborned by Pierre Guerre to accuse the prisoner of imposture. She held it in her hands to bring a judgment against Arnaud du Tilh, but she did not do so.

Here, then, using this testimony from the trial and other elements in the legal situation (my "data"), is how I reconstruct the plans that Bertrande made with her

[37] Coras, *Arrest Memorable*, 46; 59, Annotation 38. Similarly in Le Sueur, *Histoire*, Ci^{r}: "et le consentement de la femme avec laquelle il avoit cohabité quatre ans, n'estant pas croyable qu'elle eust peu si longuement estre trompée"; *Historia*, 10.

[38] Le Sueur, *Historia*, 12–13: "Mox in arcana, quae vix ullius. alii quàm marito perspecta esse possint, effusus." *Histoire*, Civ^{r}: "Passant plus oultre à ce qui ne pouvoit estre cogneu qu'au mary." Coras, *Arrest Memorable*, 63: "les plus privez et particuliers actes qui peuvent intervenir entre mari et femme"; 21; 39, Annotation 21; 44–45: Bertrande testifies first about the details of the bewitchment and then "ledit du Tilh . . . respond en tout comme ladite de Rols, sans en rien faillir, adiouster, ni diminuer"; 149, Annotation 106. Le Sueur, *Historia*, 12: "Quod antè mulier iudici exposuerat, quis rerum suarum status primis taedis extitisset: id ille, ne latum quidem unguem ab eo cursu desciscens, alacrius percurrit"; *Histoire*, Ciii^{v}: "Tout ce que Bertrande avoit auparavant desclaré separement au juge des premières choses qui estoient entrevenues entr'elle et son mary, ce faux Martin le racompta aussi de mot à mot, sans y rien obmettre."

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impostor-husband once strife had erupted between him and Pierre and the soldier had brought his news of Martin Guerre. I think they would have agreed on a policy of supportive testimony on the assumption that Pierre would be the plaintiff or, at least, the inspirer of any court prosecution, as was in fact the case in the arson-imposture trial before the Sénéchaussée of Toulouse, and that Martin Guerre, if he were still alive, would not return to France, where he could face a lèse majesté charge, since he had fought for Spain at the battle of Saint Quentin. Then, after Arnaud's arrest and imprisonment at Rieux, Bertrande had to make her own plans, with two men to placate—Pierre and the prisoner—and with her own soul, her own interest, and that of her children to consider. Arnaud clearly regretted that Bertrande had agreed to be the plaintiff against him, asking the judge of Rieux to remove her from Pierre Guerre's household and telling her gently before the Parlement that "he planned no harm against her and that her accusation was incited by the violent passion of another person," namely, Pierre Guerre.[39] In a situation fraught with risks—family feuds, a possible death sentence for him if he lost, a serious penalty for her if she were found guilty of complicity and adultery—she did her best to help Arnaud be declared her husband by the court, while protecting her own life and reputation whatever the outcome. If Arnaud had been adjudged to be Martin Guerre, as we know he almost was, he would have surely continued to lay the blame for false accusation only on Pierre Guerre and tried to resume life with Bertrande.[40]

As it turned out, Martin Guerre returned. On the last day of his life, Arnaud then did his best to help Bertrande by absolving her of any fault in his imposture. This is the kind of thing condemned men often did from the scaffold in the sixteenth century, an approved action in the scenario of execution. Husbands might absolve their wives whatever their behavior: Etienne Mayet, fuller of Touraine, freed his wife of complicity in the murder of their son-in-law, even though, we know from her confession, she had asked their servant to kill him.[41]

[39] On the sequestration, Coras, *Arrest Memorable*, 37; on Arnaud's words in court, Le Sueur, *Historia*, 12: "Reus verò contrà alacriori vultu coniugem suam compellat. nihil gravius se in eam statuere, quòd alterius intolerabili libidine niteretur. Mox in patruum invectus acerbius, quod eum universae scenae choragum nominaret." *Histoire*, Ciii^{r}: "La faux Martin . . . avec un visage asseuré et joyeux, appeloit doucement sa femme, disant qu'il ne luy vouloit aucun mal, sachant bien qu'elle l'accusoit y estant portée et incitée par autruy; et se tournant à mesdire de l'oncle du vray mary, disant qu'il estoit autheur de toute la tragédie."

[40] Not only was Bertrande in no danger from a suit from Arnaud, but, according to Alfred Soman, specialist in the history and jurisprudential practice of the Parlement of Paris, she was in little danger of a public prosecution for perjury or false testimony. Such cases were exceedingly rare before the Parlement of Paris, which means that either they were infrequently brought before the lower courts or were brought but were punished by so trivial a penalty that appeal was unnecessary. For the king's attorney to win a perjury case, "evil intent" and "malice" had to be proved, and this was virtually impossible (letter of 17 November 1986; conversation of 10 May 1988). I have found only one case of false testimony in examining many hundreds of letters of remission: a young miller of Orléans and his wife were pardoned in a case where they had changed their testimony and feared "corporal punishment," presumably, whipping (Archives Nationales, JJ238, 23^{r}–24^{v}). Coras, in discussing indices for Bertrande's innocence of complicity and adultery, defines the "danger" she was in, if her denunciation had been found "calumnious and false," not in terms of prosecution but in terms of "great shame" (*Arrest Memorable*, 141, Annotation 98).

[41] AN, JJ236, 687^{r}–689^{r}, letter of remission, dated November 1524, for Jeanne, widow of Etienne Mayet of Montbazon in Touraine, for the homicide of her son-in-law, for which her husband had been

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Arnaud's statement is an index of his attitude toward Bertrande, not an index of her complicity or non-complicity.

WHAT KIND OF A WOMAN, THEN, WAS BERTRANDE DE ROLS? Finlay describes my Bertrande as "a creature of utter calculation," a trait he considers inappropriate and anachronistic for sixteenth-century peasant women, and finds my reference to her "realism" and "stubborn independence" also unfounded and unacceptable. He perceives in my portrait other traits he might have regarded as inconsistent with "utter calculation": "passionate," "romantic," "heroic," and, worst of all, "proto-feminist."[42] This is a careless reporting of my text, its evaluation inattentive to the multiple clues about Bertrande provided in the legal sources and ill-informed about sixteenth-century rural society and about the meaning of the terms feminist, romantic, and heroic.

Let us remember the setting in the Lèze valley: women have important roles in economy and family but relatively little authority and access to property ownership and a more precarious legal personality than men. Wedded too young, unfruitfully married for years, and then abandoned without possibility of remarrying, Bertrande emerges as committed to village values, not wanting to be cut off from the sizable peasant holdings of Guerre and Rols, eager to maintain the status conferred by marriage, anxious like other village women about sexual reputation, living "virtuously and honorably" during Martin's disappearance and talking of her "honor" before the court.[43] Along with these traditional values, she also displayed on occasion a distinctive independence and willingness to take some risks to escape from her predicament.

That independence, that concern for womanly honor, and "a shrewd realism about how she could maneuver within the constraints placed upon one of her sex"[44] were first suggested to me by Bertrande's reaction to Martin's impotence and her refusal to agree with her kinfolk to the annulment of the marriage. I will review my reasoning here as an example of my method. Bertrande said in her testimony at Rieux that she and Martin were *both* "tied and bewitched" ("liez et maleficiez," plural adjectives) for eight years and could not cohabit, and then at the end of nine years, "she was freed from the spell" ("elle fut dessorcelee") and instructed to have four masses said, etc. Le Sueur puts it similarly: "Bertrande was not able to play the duties of a wife" and only after eight years "did Martin begin to play the role of the husband."[45] Since Coras's annotation on this point elaborated mostly on male impotence and bewitchment, I used a citation from the *Malleus Maleficarum* simply to illustrate how a contemporary source explained the spell on a female.

executed. The Martin de Guerre executed in Rouen in 1555 for the crime of transporting enemy letters (see below, p. 602) freed Jarmot Dechentery of complicity before he was hanged. This is a gesture found in execution scenes, one that won praise for the condemned persons who thus took responsibility on themselves, allowed for pardon for the living person, and helped their chances before God.

[42] Finlay, "Refashioning of Martin Guerre," 560, 559–61, 556, 562, 570.

[43] Coras, *Arrest Memorable*, 46; Le Sueur, *Historia*, 11; *Histoire*, Cii^{v}.

[44] Davis, *Return of Martin Guerre*, 28.

[45] Coras, *Arrest Memorable*, 40, 44; Le Sueur, *Historia*, 12; *Histoire*, Ciii^{v}.

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I then went on to suggest that, even though Bertrande would use other language, she was relieved not to have to have sexual intercourse for a while.[46]

Why? Bertrande had barely reached the age of puberty when she married. She said later that she was about nine or ten and Martin only a little older, but, whatever the case, they were very young. Demographers of sixteenth-century France have taught us how unusual such an early marriage is, especially among peasants;[47] Bertrande would not even have had a cultural preparation for so precocious a sexual union. Coras himself comments that at such a young age neither boy nor girl "can become really excited by the pricks of desire." To regard Bertrande's years of "bewitchment" as a response to this situation seems very plausible, as does her refusal to agree to the dissolution of the unconsummated marriage from fear of being forced into another marriage when she was still in her early teens—quite apart from her attitude toward the young Martin.[48] This is what I call a realistic weighing of possibilities.

I find support for my interpretation in views of peasants as characteristically weighing their choices, from the sixteenth-century commonplace of peasant "cunning" to Pierre Goubert's description, "there is something of a merchant in every peasant," to the theory of rational choice underlying Hilton Root's recent book, *Peasants and King in Burgundy*.[49] Furthermore, weighing and calculating also play a central role in the study of early modern women of different classes. These traits are an aspect of the female "prudence" found or enjoined in families of modest means or with wastrel husbands (Christine de Pizan on the wives of "foolish husbands," Charles Estienne on diligent farmers' wives). They also provide a mode of resistance for wives who, in their legal status as subject to their husbands, have few other means of getting their way. "Women feel sorry for themselves . . . and get sick when they want to," went the popular proverb; or, as a lawyer wrote to his daughter in the early seventeenth century at the start of her marriage,

> Don't do anything in or outside the house without [your husband's] advice. By obeying you'll learn to command him. I mean that when he recognizes your humble obedience, he'll no longer do anything but what you want and give over to you the free disposition of your household . . . Your late mother and I lived together in this fashion and never raised our voices to each other.[50]

[46] Davis, *Return of Martin Guerre*, 28.

[47] Pierre Goubert, *Paysans français au XVII^e siècle* (Paris, 1982), 92: eighteen to twenty is a usual age for the marriage of peasant women in the sixteenth century. Jean-François Soulet reports women as marrying "rarely" before the age of twenty in the Pyrenees: *La Vie quotidienne dans les Pyrénées sous l'Ancien Régime du XVI^e au XVIII^e siècle* (Paris, 1974), 227–28.

[48] Coras, *Arrest Memorable*, 2, Annotation 1. The phrase Le Sueur uses about Bertrande, presumably drawing from her testimony, is that after eight years "she was in despair about having offspring, when an old woman appeared as from heaven and advised her what to do." *Historia*, 12.

[49] Goubert, *Paysans français au XVII^e siècle*, 182. Hilton Root, *Peasants and King in Burgundy* (Berkeley, Calif., 1987). For the general approach to peasant culture in terms of "shrewdness" and "calculation of costs," see Eric Wolf, *Peasants* (New York, 1966), 13–17.

[50] Christine de Pizan, *The Book of the City of Ladies*, trans. Earl Jeffrey Richards (New York, 1982), Book 1, chap. 43, 87–89; Book 2, chap. 66, 209. Charles Estienne, *Maison Rustique or the Countrey Farme*, trans. Richard Surflet (London, 1606), Book 1, chap. 11, 51–53 (1st French edn., 1564); *Proverbia communia noviter aucta, revisa et emendata* (Paris, 1513), a v^v: "Femme se plaint, femme se deult, Femme est malade quant elle veult." Nicolas Pasquier, *Les Lettres* in *Les Oeuvres d'Estienne Pasquier . . . et les Lettres de Nicolas Pasquier*, 2 vols. (Amsterdam, 1723), 2: 1235–36, Book 5, letter 9. See also the article by Nicole

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The constraints placed on widows in the many wills I described from the Lèze valley illustrate well why women would be incited to ploys and maneuvers.[51] Family life has its politics just as does the Venetian state.

What is more unusual about Bertrande is her *sustaining* her refusal against her parents' pressure. Queens do this more readily, such as Jeanne d'Albret, carried rigid to the altar of her first marriage, and Marguerite de Valois, refusing to separate from Henri de Navarre against the insistence of her mother, Catherine de Médicis.[52] For a village lass in a patriarchal family, such refusal is a sign of "stubborn independence," a trait that reappeared during the months that Bertrande stood out fiercely against Pierre and her mother in defense of the impostor-husband. Indeed, even a Bertrande duped from start to finish could not be constructed as passive or easily victimized, for the evidence of her independence is unmistakable.

Finally, citing Coras's quotation about Bertrande's refusal being a "touchstone . . . of her *honnesteté*" (honesty/honor) and recalling the village women who hit and sued each other for insults to their reputation, I treat this episode as a first example of Bertrande's concern for her womanly honor.[53] She shows herself loyal to marriage and gives herself time for sexual maturation and for proving to her village that she can one day be a mother.

"Passionate" was not an adjective I used about Bertrande de Rols, although it would seem appropriate to characterize the strong feeling (the precise meaning of "passion") behind her threat that she would kill anyone who henceforth said the man living with her was not her husband Martin Guerre. My words were, "with the new Martin she had a man she could live with in peace and friendship (to cite sixteenth-century values) and in passion."[54] Is this too outrageous a conjecture from such phrases as "he lived with her for four years so peacefully" (Le Sueur), "they lived like true married people, eating, drinking, and ordinarily sleeping together" (Bertrande), and "they conversed together day and night" (Arnaud)? Does not their giving birth to two children and Bertrande's gracious reception of Arnaud when he returned to her bed from the Toulouse prison (washing his feet, giving him a white shirt, sleeping with him[55]) suggest an affectionate intimacy between a man and woman who had been strangers three years before?

Castan referred to in *Return of Martin Guerre*, 145 n. 7, for evidence from cases before the Parlement of Toulouse on how husbands and wives manipulate images of wifely irresponsibility to get themselves out of punishment.

[51] Davis, *Return of Martin Guerre*, 30–31.

[52] Nancy L. Roelker, *Queen of Navarre: Jeanne d'Albret, 1528–1572* (Cambridge, Mass., 1968), 54–55; Marguerite de Valois, *Mémoires et autres écrits de Marguerite de Valois, la Reine Margot*, ed. Yves Cazaux (Paris, 1971), 59. I speculated in *The Return of Martin Guerre*, 32, that the example of Bertrande's Basque mother-in-law may have provided an example for an independent style, given what is said about Basque women in the sixteenth century.

[53] Coras, *Arrest Memorable*, 40, Annotation 5 (*sic* for 22). On concern about woman's honor in the Artigat region, see the case cited in *Return of Martin Guerre*, 32, and among peasant women more generally, see Natalie Zemon Davis, *Fiction in the Archives: Pardon Tales and Their Tellers in Sixteenth-Century France* (Stanford, Calif., 1987), 95–101.

[54] Davis, *Return of Martin Guerre*, 44.

[55] Le Sueur, *Historia*, 7; *Histoire*, Bii^{v}. Coras, *Arrest Memorable*, 25, 149, 68.

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But Bertrande was also a woman of conscience. When faced with too much pressure or danger, as she was during the lonely months of sequestration and imprisonment during the trials, she could be uncertain about the rightness of her actions. This is one of the interpretations I placed on her conduct during the Toulouse trial: the lowered eyes and voice full of trepidation as she testified (indicating unease about both sides of her double game) and the "trembling like a leaf" and weeping before the returned Martin Guerre. Yes, the weeping, which Finlay curiously reads me to say was contrived, I saw, on the contrary, as quite genuine, born of terror and guilt.[56] It is her words to him that seem to me a prepared excuse, for example,

As soon as [I] began to perceive the fraud, [I] wanted death many times over . . . and seeing this trickster had stolen [my] honor and [my] reputation for chastity, [I] *immediately* brought him to justice [emphasis mine].[57]

"Romantic" this portrait certainly is not, Bertrande's love seasoned as it was by pragmatism, self-interest, and concern for her children. Nor is this portrait "heroic," Bertrande accusing her preferred impostor-husband in order to satisfy Pierre Guerre and accommodating herself to the returned husband who had deserted her. "Feminist" least of all, for there is no sign in her maneuvering or adventuresomeness of an argument for changing the lot or image of peasant women in a fundamental way, no sign of an argument about woman's education or woman's moral potential, which we associate with the early feminists Christine de Pizan, Cornelius Agrippa, Louise Labé, and Lucrezia Marinelli. (Le Sueur even claims the weeping Bertrande told Martin that she had accepted Arnaud "out of imprudence and the inconstancy of women."[58]) Bertrande, in my *Return of Martin Guerre*, can be located along a spectrum of ordinary peasant women, sometimes accepting, sometimes resisting their personal fate, the only possible cultural novelty in Bertrande's life being that of Protestantism, of which more in a moment.

MY PORTRAIT OF ARNAUD DU TILH IS A DOUBLE ONE, involving both a talented trickster with gifts of memory and tongue, who spent a youth of mischief, loose living, and soldiering, and a man who embraces his role as the husband of Bertrande de Rols and the heir of Sanxi Guerre. The first part of my portrait Robert Finlay accepts, the second part he finds a whitewash, "elevated and exculpatory."[59] An impostor is an impostor, an adulterer is an adulterer, and there is nothing more to say.

The argument for Arnaud du Tilh's remaking himself as Martin Guerre was prompted by three kinds of evidence about him. First is the reported transformation of his behavior from the "dissolute" young man "of bad life" from Sajas. "For the prisoner did not appear thus," commented Coras. He lived those years

[56] See Coras, *Arrest Memorable*, 100; Le Sueur, *Historia*, 16; *Histoire*, Diiᵛ. Coras says she ran to embrace him; Le Sueur says she threw herself at his feet. Finlay, "Refashioning of Martin Guerre," 560.
[57] Coras, *Arrest Memorable*, 102, 105.
[58] Le Sueur, *Historia*, 16: "imprudentia et muliebri inconstantia."
[59] Finlay, "Refashioning of Martin Guerre," 564.

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in Artigat "with no offense," commented Le Sueur, "conducting himself well in all affairs."[60] Second is the fact that Arnaud never left, never took the money and ran, but remained embedded in the economic life of the Lèze valley and in the rhythms and quarrels of the Guerre household. Third, and especially telling, is his behavior before the court after the man with the wooden leg had been identified as Martin Guerre—by Bertrande, by Pierre, and by the sisters, who had until then remained the prisoner's staunch allies. Arnaud refused to renounce his claim to be Martin Guerre, continuing his "impudence" before the Parlement, interrupting President Mansencal as he read the final sentence,[61] and taking his birth-name back only four days later, the morning of his execution in Artigat. This conduct before the court, when all hope was irrevocably lost, suggests the strength of his self-identification as the true husband of Bertrande de Rols.

Within this frame, I tried to make sense of Arnaud's economic actions, for example, of his selling some Guerre properties in Artigat and leasing one in the Basque Hendaye. Pierre thought this was wasting the patrimony, but the impostor's supporters among the villagers of Artigat evidently did not perceive him as recklessly alienating the Guerre holdings. Arnaud's behavior was surely imprudent in violating Basque custom in regard to inheritance property and in defying the old Basque uncle, legal guardian of the patrimony until Arnaud took Martin's place, but it was in accord with the busy land market and commercial expansion revealed in the archives of the Lèze valley and known more generally for Languedoc in these decades.[62] This is "theft," as Coras named it,[63] for Arnaud du Tilh had no legal right to sell Martin Guerre's lands, but it is the theft of a man with a double memory, staying put and reinvesting Guerre wealth rather than siphoning it off to the du Tilhs. A theft is a theft, but judges distinguish among them in giving sentence, and historians ought to make even greater distinctions if they want to give rightful account.

Descriptions of Arnaud's behavior toward Bertrande during the trial also suggest a genuine affection for her: "with a face assured and joyous, he called his wife gently, saying he wished her no harm" (Le Sueur); he swore that he would "submit to a thousand cruel deaths if she would swear he were not her husband" (Coras). Together with his "living peaceably with her" and his statement to Martin from the scaffold, entreating him not to mistreat Bertrande, because she was a

[60] Coras, *Arrest Memorable*, 77–78, as argument why the prisoner could be the real Martin Guerre: "N'y fait rien aussi, que ledit du Tilh ay esté des sa ieunesse dissolu, de mauvaise vie, et addonné à toute espece de meschancetez: car il n'appert point que le prisonnier soit celuy la, ains plus tost Martin Guerre." (Of course, it should be remembered that Martin Guerre had left home because of a "theft" of grain from his father.) Le Sueur, *Historia*, 7: "sine ulla offensione vixerit." *Histoire*, Bii^{v}, "et se conduisant si bien en toutes affaires, qu'on n'eust pu avoir de luy aucun soupçon de mal."

[61] Le Sueur, *Historia*, 18; *Histoire*, Div^{v}: "Toutes les choses remonstrées par le President, en grande assemblée de Messieurs du parlement, ce faux Martin ne changeoit aucunement son asseurée impudence . . . Aucuns furent d'advis qu'on luy baillast la gehenne pour en tirer enclore la verité par sa confession." Coras, *Arrest Memorable*, 128, Annotation 90: "ce temeraire estoit d'une impudence effrontée . . . et eut à chaque mot troublé monsieur le president qui prononçoit [l'arrest]."

[62] See the reference to sales contracts and the citation to Emmanuel Le Roy Ladurie's study of the Languedoc in *Return of Martin Guerre*, 142 n. 2.

[63] Coras, *Arrest Memorable*, 126.

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woman of "decency, virtue and constancy,"[64] Arnaud emerges as a man who took seriously his years as Bertrande's substitute husband.

Among the terms I used for Arnaud du Tilh's partial transformation from trickster to husband, as well as for the adventures of Martin Guerre and the endeavors of Bertrande de Rols, was "self-fashioning." I referred to Stephen Greenblatt's brilliant development of that term and to an allied concept in Norbert Elias's *Civilization of the Body*, both helpful because they link changes in conduct and self-consciousness to the elaboration of the state.[65] Finlay fancies this a twentieth-century literary trend. On the contrary, not only is the "individual as work of art" a commonplace of Renaissance scholarship since Jacob Burckhardt's day, the term "self-fashioning" derives from Montaigne, from the essay I cited in my next sentence: "On s'y forme, on s'y façonne . . . car la dissimulation est des plus notables qualitez de ce siècle" ("Men form and fashion themselves . . . , for dissimulation is among the most notable qualities of this century").[66] Or, since Finlay is a specialist in Italian history, he might recall the theme of "fashioning" from Castiglione's *Book of the Courtier* or from the *Galateo*, the much reprinted book of manners of the Venetian Giovanni Della Casa.[67] "Self-fashioning" is almost as old as "the myth of Venice."

I introduced the term for two reasons. First, I wanted to account for the fascination of the Martin Guerre case to the many learned men who wrote about it and commented on it, starting with Coras himself. Men of letters were preoccupied with masking, with false face and language: in Roman times, said the Toulousain Guillaume de La Perrière, masks were worn for special occasions, now they are worn every day. "For dissimulation is not bad," said lawyer Philibert de Vienne in his satirical *Philosophe de Court*, "not a wrong, nor fraud . . . for we do it to please others . . . an easy way to attract benevolence, honor, and good reputation."[68] Here is a link between "the thousand necessary lies" of the peasant

[64] Le Sueur, *Historia*, 11–12; *Histoire*, Ciii[r]; Coras, *Arrest Memorable*, 66; Le Sueur, *Historia*, 21: "Martinum obstestari ne alieniore à Bertranda (quam ut sibi ignosceret unicè rogabat) esse animo, quòd eius pudori vitium intulisset. Eam nul li pudore, virtute et constantia cedere." *Histoire*, Eii[v]: "il prioit Martin ne vouloir faire aucun mauvais traitement à Bertrande, pour ce qu'elle n'avoit aucun coulpe pour ce qui s'estoit passé, mais qu'elle estoit une fort honneste et prude femme."

[65] Davis, *Return of Martin Guerre*, 103, 152 n. 6.

[66] Michel de Montaigne, *Essais*, Book 2, chap. 18: "Du Dementir" in *Oeuvres complètes*, eds. A. Thibaudet and M. Rat (Paris, 1962), 649 (cited in *Return of Martin Guerre*, 152 n. 15), English trans. by Donald Frame, *The Complete Works of Montaigne* (Stanford, Calif., 1948), 505.

[67] Baldassare Castiglione, *The Book of the Courtier*, trans. Thomas Hoby (London, 1948), especially Book 1. Hoby says, "Let us fashion such a Courtier" (16) for the Italian "formiamo un cortegian." See the study by Wayne A. Rebhorn, *Courtly Performances: Masking and Festivity in Castiglione's "Book of the Courtier"* (Detroit, Mich., 1978), which has as one of its goals "tak[ing] seriously Castiglione's image of the self as a work of art and find[ing] in the principles and practices of High Renaissance portraiture the key to characterization in his book" (18). On editions of the *Galateo*, see Antonio Santosuosso, "Books, Readers, and Critics: The Case of Giovanni Della Casa, 1537–1975," *La Bibliofilia*, 79 (1977): 101–86. Is there no self-fashioning involved in the processes by which young men were enticed, persuaded, or compelled to become decorous adult members of the Venetian regime? See Stanley Chojnacki, "Political Adulthood in Fifteenth-Century Venice," *AHR*, 91 (1986): 791–810.

[68] Guillaume de La Perrière, *Le Theatre des Bons Engins* (Lyon: Jean de Tournes, 1549), A8[r]; Philibert de Vienne, *Le Philosophe de Court* (Lyon: Jean de Tournes, 1547), 63. Vienne was a lawyer in the Parlement of Paris.

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Arnaud du Tilh and the constant shaping of word and gesture in the world of favor and royal advancement.[69]

Second, I wanted to extend the concept of forming and fashioning the self to a wider range of situations and social groups—to make it a sixteenth-century issue, not just a "Renaissance" issue. Thus the book tracks a number of peasant migrations, where people must change their language and manners—opening with the Daguerre removal from the Basque country to the Léze valley, to Martin Guerre's movement to Spain, to Arnaud du Tilh's arrival from his Gascon Sajas in the langue d'oc-speaking Artigat. I explore the customs of nicknaming and carnival masking in these regions and then cross into the transgression of taking on a false name in forgery cases, in stories, and finally in the case of Arnaud du Tilh. "Imposture" stands not as an isolated form of behavior, not as a disconnected "monstrosity" or disjoint "prodigy," but as an extreme and disturbing case on a sixteenth-century spectrum of personal change for purposes of play, of advantage, or of "attracting the benevolence of others."[70]

As I embedded "imposture" in the cultural practices of sixteenth-century life, so I sought to embed what I called "the invented marriage"—the relationship that began with a false identity but was sustained by collaboration—in some kind of cultural understanding available to sixteenth-century peasants. What traditions or ways of thinking, I asked, might they have clutched at to make an invented marriage seem to them practical or even rightful in light of the long disappearance of the real Martin Guerre? I suggested two answers: the centuries-old custom of clandestine marriage, condemned only in 1563 at the Council of Trent, by which the sacrament of marriage is performed simply by the man and woman exchanging "words of the present" and tokens;[71] and the newer Protestant teaching, by which a wife deserted by a husband is free to remarry after a year. I might well have added a third social practice, that of bigamous unions.

As for Protestantism, I establish beyond the shadow of a doubt the presence of converts, pastors, and religious action in the region around Artigat in the late 1550s, a good decade before the inhabitants of Artigat cleansed the church and were described in a local manuscript as "Huguenots." The question is whether we can see any signs of Protestant sentiment in Bertrande and Arnaud. The closest evidence concerns Arnaud. (Finlay's comment that his blaspheming youth "does not bespeak receptivity to the Reformed religion" shows how unfamiliar he is with the conversion patterns of sixteenth-century Protestantism.[72] The conversion of the former gambler, sinner, blasphemer, and fornicator is a topos of Reformed

[69] Coras, *Arrest Memorable*, 12, Annotation 5.

[70] Vienne, *Le Philosophe de Court*, 63. See a like distinction in meanings of "prodigious" in the sixteenth century in Davis, *Return of Martin Guerre*, 106, and the book by Jean Céard, cited in 152 n. 4.

[71] Robert Finlay refers to this incorrectly as a "non-sacramental view of marriage" ("Refashioning of Martin Guerre," 563). Clandestine marriage was disapproved of by the church but was, as I point out in *Return of Martin Guerre*, "an indissoluble union" (46), valid, and thus sacramental. Only with the 1563 bull *Tametsi* does it change its status. To the texts cited on 141 n. 7 can now be added Charles Donahue, Jr., "The Canon Law on the Formation of Marriage and Social Practice in the Later Middle Ages," *Journal of Family History*, 8 (Summer 1983): 144–58.

[72] Finlay, "Refashioning of Martin Guerre," 564.

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martyrologies like Jean Crespin's *Histoire des martyrs* and a fact of numerous Protestant lives. It is, of course, a commonplace in Catholic conversion stories as well.) To begin with, there is the respect accorded Arnaud for so long during the trial by the Protestant Jean de Coras. Coras's Protestant sympathies were already in place by 1560; a recent essay by Aldo Stella has shown him being denounced as early as 1534, during his student days at Padua, as "a heretic, banished for heresy from the city of Toulouse."[73] Could his finding the false Martin believable have been sustained not merely by the prisoner's perfect memory, repeatedly praised by Coras, but by his giving off in those many interviews the religious cues that the judge took seriously?

Whatever the case, Coras's description of Arnaud's end, added to the 1565 edition[74] at a high point of religious controversy, would sound Protestant to contemporaries, lacking the characteristic Catholic invocation to Mary and all the saints of the celestial court of Paradise: he was executed "asking pardon . . . with great signs of repentance and detestation of his deed, crying always mercy to God by his son Jesus Christ." Le Sueur's quotation is even closer to Protestant formulas, and he, too, was a supporter of the Reformed cause: "For all the things with which he had . . . defiled himself most foully in body and soul, he asked grace from God, who does not spurn sinners if they turn toward his divine will and place their hope in the blood of Christ, shed for the salvation of everyone."[75] Why would two Protestants represent a condemned criminal praying in such a fashion if they did not intend to suggest a Reformed sensibility that had led him finally to repent?

The Protestantism of Arnaud and Bertrande remains a mere possibility throughout my text, never a certainty, yet one that suggests connections—and ways to think about connections—between the hard-to-document attitudes of individual peasants and major cultural movements of their day. When Finlay says "there is no warrant in the sources for introducing a religious dimension to the Martin

[73] Aldo Stella, "Atteggiamenti eterodossi di studenti francesi nell'università di Padova," in *Scambi culturali tra l'Italia e la Francia dal Medioevo all'età moderna* (Geneva, 1987), 289–91. In a 1982 paper for a graduate seminar directed by Professor Ralph Giesey of the University of Iowa, Gillian S. Gremmels explored Coras's use of the Bible in the *Arrest Memorable*. She finds evidence of Calvinist sentiments in the text but points out that he was not a careful biblical scholar and that he was not using a Geneva Bible. He could have been using one of the vernacular editions published in Lyon, however, which prudently avoided some of the obvious Reformed signs (Bettye Thomas Chambers, *Bibliography of French Bibles: Fifteenth- and Sixteenth-Century French-Language Editions of the Scriptures* (Geneva, 1983), 144 *et passim*.

[74] Jean de Coras, *Arrest Memorable du Parlement de Tolose, Contenant Une histoire prodigieuse, de nostre temps, avec cent et onze belles, et doctes annotations: dont les onze ont esté nouvellement adioustees sur le procez de l'exectuion dudit Arrest* (Lyon: Antoine Vincent, 1565). Finlay cites only the 1561 edition (n. 9) and evidently did not consult the 1565 or later editions.

[75] Coras, *Arrest Memorable*, 160. Le Sueur, *Historia*, 21: "Quorum omnium, atque eorum, in quibus foedissimè tota vita volutatus, quibus et animo et corpore turpissimè conspurcatus esset, à deo opt. max. qui ne profligatos quidem, si ad eius numen se converterint, si in Christi cruore ad salutem omnium profuso spem collocarint, solet aspernari, veniam postulare." *Histoire*, Eii^{r-v}: "Dont et de toutes les mechancetez qu'il avoit commises en sa vie il requeroit à Dieu pardon lequel il sçavoit bien recevoir à sa misericorde tous les pecheurs qui s'addressent à luy avec repentance." Among other differences, Catholic formulas would call on Mary, one's name saint, and all the saints in the celestial court of Paradise. This is the case with Catholic wills in Artigat and its region (see, for example, Archives départmentales de l'Ariège, 5E6219, will of 31 July 1530; 5E6859, will of 3 May 1541; 5E6653, 3^{r-v}.

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Guerre story,"[76] he makes several mistakes. He fails to recognize what clues there are in Coras and Le Sueur, and he fails to recognize as a "source" the facts about the local religious movements impinging on the lives of all the actors. (This is the eve of the French Religious Wars!) More important—and quite apart from my Protestant speculation—he does not think it interesting to ask about what goes on in the heads of peasants. How can there not be religious dimension in the sixteenth century to a case concerned with sexuality, lying, marriage, and the definition of self? Finlay can talk about the doge's procession as "commemorat[ing] Venice within the context of providential design"[77] but not think it worth reflecting on how Arnaud and Bertrande see themselves within a providential design. For him, that would be elevating a story better left to the idiosyncrasies of persons of low estate, to the fraud of the impostor and the fidelity of the wife.

ARNAUD DE TILH WAS CONDEMNED BY THE PARLEMENT OF TOULOUSE to be hanged and burned for his "imposture and false supposition of name and person and adultery"; Bertrande de Rols, along with Martin and Pierre Guerre, was freed from prosecution and punishment.[78] Does this mean, as Finlay claims, that the court never suspected that Bertrande was guilty of adultery and/or that at some point she had been Arnaud's accomplice? Does it mean that the Parlement's decision was made without any lingering doubts about "what Bertrande knew"? The answer is no, once one has taken careful notice of Coras's text and also placed the Parlement's verdict within the frame of sixteenth-century French juridical practice. Here, then, we turn our historian's argument from what Bertrande knew to "what the judges knew."

The decision about whether or not to prosecute Bertrande was, says Coras, the most difficult the court had to make in its final deliberations. First, he characterizes the court's concern about both Martin and Bertrande: "The greatest difficulties in judging this trial and on which the court worked the hardest were whether Martin Guerre and Bertrande de Rols should be condemned." After weighing the pros and cons in regard to Martin (his abandonment of his family, his not communicating with them, his serving in the Spanish army against France), he goes on, "Even greater, certainly, was the difficulty in regard to Bertrande de Rols."[79]

[76] Finlay, "Refashioning of Martin Guerre," 564.

[77] Finlay, *Politics*, 122–23.

[78] Archives départementales de la Haute Garonne, B, La Tournelle, 76, 12 September 1560: "Et pour punition et reparation de l'imposture et faulce supposition de nom et personne et adultere par ledit du Tilh," he is condemned to make an amende honorable, etc. "Et en oultre [the court] a miz et mist hors de proces et instance ledit Martin Guerre, ladite Bertrande de Rolz, ensemble ledit Pierre Guerre, oncle dudit Martin."

[79] Coras, *Arrest Memorable*, 135, Annotation 97: "Les plus grandes difficultez du iugement de ce procez et ausquelles la cour se travailla le plus furent si Martin Guerre et Bertrande de Rols estoient en voye de condemnation"; 138, Annotation 98: "Plus grande certes estoit la difficulté pour le regard de ladite Bertrande de Rols." Finlay says, "Davis presents no evidence for her contention that 'after much discussion' about Bertrande, the judges of Toulouse" decided not to prosecute her (557); the reader can see from this quotation, its page number duly cited (*Return of Martin Guerre*, 149 n. 20), that I was paraphrasing Coras's text. This is one of several places where my notes were "beside the point" for Finlay because he was inattentive to what was on Coras's page.

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He gives several reasons for that difficulty. Bertrande accepted the impostor "much too easily" and "imprudently." But could this be the whole story? Why did she live with him in intimacy for three years and defend him as her husband *even after* those whom she had reason to trust said he was not?[80] Elsewhere in his annotations, Coras comments that in other impostures "women have often shown themselves sharper, quicker and more perspicacious than men in uncovering the fraud."[81] So unlikely did it seem that Bertrande could not tell the difference that the court was led to believe the accused was in fact Martin and Bertrande was being forced to testify falsely by Pierre Guerre: "in such a long interval, it is not plausible ['vraisemblable'] that she would not have recognized him as a stranger if he had not been truly Martin Guerre."[82] If this is the way the court was thinking about the wife before the return of the real Martin Guerre, what does it imply about the court's thinking after his return, when they were deciding whether to punish her or not? It implies at least two plausible theories about Bertrande, and the court talked about both: that she was tricked for three years and that she was a knowing participant in adultery.

"Adultery" is a word that surfaces in Coras's consideration of whether Martin Guerre deserved a punishment for abandoning his wife for such a long period of time and giving her no word of his existence:

> The husband's long absence rendered him *de prime face* extremely guilty and almost beyond any excuse, as having given the occasion for the misfortune and disaster ["malheur et desastre"] that followed. Witness Saint Augustine when he says, "if you abstain from your wife for a long time without her volition, you give her license to have sex where she should not ['paillarder'], and, if she conducts herself badly, her sin will be blamed on your abstinence." From which opinion, some have dared to say that a judge could justly absolve a wife accused by her husband of adultery ["d'adultere"], if she is convicted of having abandoned herself and her honor only after having been left for a long time by her husband, who gave her the occasion and thus made himself guilty of the wrongdoing.[83]

[80] Coras, *Arrest Memorable*, 138, Annotation 98.

[81] Coras, *Arrest Memorable*, 12, Annotation 5: "Bien qu'en autres suppositions les femmes se soyent monstrées plus aigües, vives et perspicaces à les descouvrir et cognoistre que les hommes." His examples are the wife of Q. Sertorius at Rome and Jeanne, daughter of the Count of Baldwin. He returns at length to Jeanne's unmasking of the impostor pretending to be her father on 117, Annotation 82. In addition to queries centering on Bertrande's knowledge and intention, Coras gives two other arguments that could lead to condemnation but that weaken or put aside the question of intention: had not Augustine found Lot guilty, not of incest with his daughters but of letting himself get drunk? Could it not be said that, even without Bertrande's intention, "the action was so prodigious and bad, the adultery so pernicious in example, that it should be punished in itself"? Coras puts aside these arguments in favor of those focusing on knowledge and intention (138–40, Annotation 98).

[82] Coras, *Arrest Memorable*, 59, Annotation 38.

[83] Coras, *Arrest Memorable*, 5, Annotation 2, referred to again 136, Annotation 97, when Coras is reviewing the other side of the argument, that is, what can excuse Bertrande from guilt: "what little fault there be, if any, should be imputed to Martin Guerre, having remained absent for so long a time, rather than to Bertrande de Rols, because of what I said above following Augustine." The text cited from Augustine is *De Conjugiis Adulterinis ad Pollentium, Libri Duo*, where Augustine comments on Matthew 5:32: "Whoever shall put away his wife, saving for the cause of fornication, causeth her to commit adultery." Augustine, "Les Deux livres à Pollentius sur les mariages adultères," Book 1, chap. 2; Book 2, chaps. 10, 17, in *Oeuvres complètes*, trans. Péronne, *et al.*, 33 vols. (Paris, 1869–78), 29: 591, 625, 634. The canon law text he was citing was the *Alphabetum aureum* of the Petrus Ravennas (d. ca. 1509).

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Coras was not happy about this last formulation, taken from a late fifteenth-century canon lawyer, since such a view could give license to wifely wrongdoing "under the cloak of her husband's absence." But the discussion makes clear (in contrast to Finlay's claim that "Bertrande's good faith was . . . never a matter of debate"[84]) that Coras and his fellow judges wondered long and hard about Bertrande's possible complicity.

Does the final verdict signify an end to all their doubts? They decided to accept her statement, made by a woman of otherwise honorable and virtuous reputation (an important criterion for judges in establishing credibility), that Arnaud had deceived her for three years and more. Apart from Martin, no one in the Guerre family had accused her—or each other—of complicity. Even if she had too easily believed Arnaud's signs, others had done so as well, Martin had been gone for long years, and she wanted her husband back; her error had come from no ill intention.[85]

But the Bertrande in error that emerges alongside the collaborating one in Coras's text is not a simple victim. The first phrase the judge uses to account for Bertrande's being tricked is a standard legal formula, one that draws on long-term cultural definitions of male and female and is also conveniently at hand whenever courts want to mitigate female responsibility: "To excuse the said de Rols comes first into consideration the weakness of her sex, easily deceived by the trickery and finesse of men, to whom the law does not presume intention to do wrong."[86] We see a formula of this kind employed in a 1545 royal letter of pardon for Jeanne Domecourt, native of Wallers in Picardy and inhabitant of Saint Quentin, for her imposture over several years of Damoiselle Charlotte Lebel, the heiress of a small fief and other properties in the village of Esmery. Jeanne confessed the story fully, starting with her being mistaken for Charlotte, describing how she fooled Charlotte's widowed mother (with whom, however, she did not live and visited only once), and how she acquired and sold the fief. Her excuse was that the imposture had been wholly the idea of Messire Pierre de Vaulx, canon of the church of Saint Quentin, whose servant and concubine she had been and "whom she could not rightly disobey." The letter remitted her penalty of banishment forever from the kingdom of France because "the fault of the suppliant had come about through the fragility of the female sex, she being in the power of . . . her master, and being [at the time] a minor under twenty-two years of age."[87]

A similar mitigation of female responsibility emerges in decisions by the Parlement of Paris in cases of bigamy. Alfred Soman finds in 1572–1585, for instance, that of roughly seventy men appealing bigamy sentences, between a third and a half were condemned to death, and almost all of the rest were given some kind of corporal punishment, such as the galleys and whipping; of the twelve

[84] Finlay, "Refashioning of Martin Guerre," 557.

[85] Coras, *Arrest Memorable*, 140–41, Annotation 98.

[86] Coras, *Arrest Memorable*, 139, Annotation 98: "Pour l'excuse de ladite de Rols Vient premierement en consideration la foiblesse de son sexe, facile à estre deceu par l'astuce, calidité et finesse des hommes."

[87] AN, X^{2A} 98, 31^{v}–36^{r}. The Parlement of Paris ratified Domecourt's letter of remission in the spring of 1546, ordered her release from prison, and requested the bishop of Noyon to start a prosecution of the canon before a spiritual court.

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women appealing bigamy sentences from 1564 to 1588, one was hanged, four were released, and the others were whipped and/or banished. In a 1610 case in which both members of the bigamous couple were punished, the husband was hanged while the wife was whipped and banished for nine years.[88]

Thus Bertrande's being freed by the court from prosecution and punishment should be placed within a grid of judicial practice where women involved in imposture and bigamy (and in other crimes as well, apart from infanticide and witchcraft) were punished less seriously than their male collaborators. The decision also served the court's commitment to family coherence, to keeping the returned Martin with his wife and children—"In doubtful situations the law commands that the presumption in favor of marriage triumph over any other," wrote Coras[89]—and to local pacification, so important in the judicial ratification of letters of pardon.

Indeed, the freeing of Bertrande in some ways resembles a pardon. Coras devotes an annotation to pardonable crime after he quotes Bertrande's supplication to Martin "to pardon her fault, committed by imprudence and because she was overcome by the seduction and ruses of [Arnaud] du Tilh." Her wrongdoing was pardonable as involuntary, but, in the shades of excusability that Coras considers in regard to Bertrande—from "error without good foundation" to fully understandable error—President Mansencal extracted the middle position of Bertrande's "having believed too lightly."[90] Reconciling Martin and Bertrande publicly before the court, Mansencal blamed Martin for having left his young wife for so long and then said, in the report of Le Sueur, that "Bertrande in truth was not free of guilt, for she had too easily allowed herself to be led into fraud and had persevered in it too long a time. And he ordered her to ask pardon of her husband. So it came about that they agreed to forget their injuries and to accept reconciliation in holiness."[91]

READERS MAY NOW BE ABLE TO SEE why Coras's *Arrest Memorable* can be said to tell two stories at once. It is both a legal narrative and a literary interplay between a TEXT and ANNOTATIONS (Coras's terms). In its legal aspects, it moves through the entire case, starting with summaries of Bertrande's brief and then of Arnaud's brief, presenting and annotating the arguments for each side. The middle section of the book produces the pros and cons that Coras and his fellow commissioner

[88] The punishments for men involved in bigamy are given in Alfred Soman, "Les Procès de sorcellerie au Parlement de Paris (1565–1640)," *Annales: Economies, sociétés, civilisations*, 32 (1977): 797, Fig. 4, and he comments on 799, "nombreux étaient les bigames masculins à être exécutés, alors que les femmes s'en tiraient le plus souvent par l'amende honorable ou le fouet." I am grateful to Dr. Soman for providing me with his unpublished material on the punishments for women appealing bigamy sentences, collected from the registers of the Conciergerie du Palais at the Archives de la Préfecture de la Police.

[89] Coras, *Arrest Memorable*, 70–71, Annotation 50; and the same point, 59, Annotation 38.

[90] Coras, *Arrest Memorable*, 100–01, Annotation 75, and 139–41, Annotation 98. On pardon in sixteenth-century France, see my *Fiction in the Archives*.

[91] Le Sueur, *Historia*, 19: "Bertrandam verò non vacare culpa: quae tam facile in fraudem induci se passa, in ea tandiu perseverasset. Iussa à marito veniam postulare. Quo contigit ut iniuriarium oblivionem, et ad extremum spiritum conciliationem sanctissime reciperent." *Histoire*, Ei[r].

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had to think about in preparing their report: arguments that the prisoner is Arnaud du Tilh or that on the contrary he is Martin Guerre. At the point where the court was "in great perplexity" but "more disposed to the advantage of the prisoner" (Annotation 68 of the 100 annotations that appear in the first edition), the real Martin Guerre appears. The internal debate continues: for example, all the pros and cons about Bertrande's culpability are considered once again, grouped in Annotation 98. The book has conclusions, that is, the judgment that decides between the arguments and names the man to be executed and those to go free. But the reader is left with the reasoning and evidence from which to construct alternate views.

Other features of the *Arrest Memorable* also lead to the doubling of a text that, for Finlay, must always be single-minded, objective, and transparent. When a sixteenth-century writer uses words like "prodigious," "tragedy," and "tragicomedy," the historian must pay attention, for they are hot and expanding terms of that day. "Prodigious" helps produce a moral tale centered on the trickster Arnaud and, as I put it, his "full dazzling power of deception";[92] "prodigiousness" requires a duped and innocent Bertrande. "Tragedy" and "tragicomedy," applied as they are to the actions of mere villagers, Arnaud du Tilh and Bertrande de Rols, help produce a counter-story that invites sixteenth-century readers to identify with their lot. Martin Guerre is sent by God's providence at just the right moment in either account to demonstrate the weakness of the judge's human reason, but in one case his return unmasks a magician-adulterer, in the other it interrupts lovers caught in a fateful lie. Here, life is being seen through the prism of art, not the prism of the villagers of Artigat but that of the *Arrest Memorable*.

Similarly, when a sixteenth-century judge misstates the actual crimes for which the man he has sentenced has been condemned (Coras adds to the crimes of imposture and adultery recorded in the decree in the parlementary register the words "abduction, sacrilege, plagiat, and larceny"), the historian asks what that means. I say he is trying to defend his decision for the death penalty, which we know had its critics—in fact, he says it himself—while the annotation on "raptus" (abduction, here, for purposes of sexual violation) ends "from this emerges a kind of excuse for the said de Rols, for a forced woman cannot be reproached with adultery."[93] When a judge comments that the burning of the body of a criminal like Arnaud is justified "so that the memory of so miserable and abominable a person be annihilated and lost"[94] and then publishes an account of such a criminal

[92] Davis, *Return of Martin Guerre*, 109.

[93] His long annotation on the crime of "supposition du nom" ends saying that it is up to the discretion of the judge to weigh the circumstances and decide whether the death penalty should be given. Then he continues, "but in the case of our Arnaud du Tilh there are so many capital crimes assembled, each one worthy of the death sentence that there is not much reason to wonder about it any more, as we will now go on to show" (*Arrest Memorable*, 118, Annotation 82). "Raptus" is a capital crime in Roman law in its various forms; for the other three crimes, Coras specifically insists that they are worthy of death (125–27, Annotations 85–87). Finlay keeps forgetting that the *Arrest Memorable* is first and foremost a legal text, written by a great specialist in the Roman law. On Coras as a scholar of the law, see A. London Sell, *Origins of Legislative Sovereignty and the Legislative State*, 3 vols. (Königstein and Cambridge, Mass., 1983–87), esp. vol. 1, *Corasius and the Renaissance Systematization of the Roman Law*.

[94] Coras, *Arrest Memorable*, 132, Annotation 94.

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under the title *Memorable Decree*, the historian asks what that means. I say it means giving Arnaud's story another chance to be heard and debated by readers.

And when a jurist-author invents a new kind of book—for the *Arrest Memorable* is a new kind of writing on crime and the law—the historian wonders why. I offer several reasons, including Coras's recently developed interest in publishing in French, but the one that arouses Finlay's ire the most is the suggestion that Coras identified on some level with Arnaud du Tilh. I urge parallels between the two lives, which Coras could have perceived, for example, the remarkable rhetorical skills of both men and the rebelliousness of both men, so problematic for Coras, himself both Protestant critic and defender of royal sovereignty. Aldo Stella's recent finds on Coras's student days reveal yet another one: as Arnaud was suspected of magic in his youth (reported by Coras), so Coras's circle at Padua was accused of owning "magical and illicit books."[95] As Arnaud's talents could be used for good or ill, could also those of Coras?

I am thus engaging in the historian's common practice of conjecturing from evidence on the basis of assumptions about psychological process. The processes assumed here—of identification and ambivalence—are simple ones and quite transportable across five hundred years of West European history; Rabelais has equivalent ways of talking about the same thing. Finlay discusses neither evidence nor assumptions but just snorts that they are "unfounded." I leave it to readers to weigh my argument.

ROBERT FINLAY HAS NO DOUBTS ABOUT WHAT IS TRUE in the case of Martin Guerre—"no reader of Coras can doubt that the court and everyone else concerned had discovered the truth of the matter"—but I think Jean de Coras had a few, and certainly some of his sixteenth-century readers did.[96] The most important of them was Michel de Montaigne, whose essay "On the Lame" is the heart of my last chapter. Against Finlay's sense of closure, let me once again quote Montaigne, who arrives at Coras after reflecting on the dangers of credulity and the uses of being tentative about what one does not know: "Me thought he proved his imposture, whom he condemned as guiltie so wondrous-strange and so far-exceeding both our knowledge and his owne, who was judge, that I found much boldness in the sentence, which had condemned him to be hanged."[97] Better to have followed the example of the Areopagus Council of ancient Athens, which, when it was perplexed, told the parties to come back in a hundred years. Four hundred years

[95] Davis, *Return of Martin Guerre*, chap. 10, esp. p. 96. Coras, *Arrest Memorable*, 90. Stella, "Atteggiamenti," 290–91.

[96] Finlay, "Refashioning of Martin Guerre," 567–68. The request that Coras add information to a new edition of his book about Arnaud's finally confessing to his imposture was made "at the continual supplication and prayers of his well-wishers" (Advertissement de l'Imprimeur aux Lecteurs, * ii^{r}, from the 1572 edn.). The title page of the 1565 edition specifically announced that it had added the information "sur le procez de l'execution." This sounds to me as though Coras's readers wanted to know what happened after the sentence was read.

[97] Montaigne, "Des Boyteux," in *Essais*, Book 3, chap. 11, *Oeuvres*, 1008, trans. by John Florio, *The Essayes . . . of Lord Michael de Montaigne* (London, 1610), "Of the Lame or Cripple," 615.

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later, I think the Martin Guerre case still an excellent one to teach us humility in our rightful quest for historical truth.

The rights and wrongs in the Guerre case are also crystal clear to Finlay, and he attributes a similar moralism—a wrongful and romantic moralism—to me. "If readers of her book feel a kinship with Bertrande and lament the return of the man with the wooden leg . . ."[98] (Is he confusing the book with the film, I asked myself in reading this sentence? A British feminist reviewer thought my epilogue unrealistically soft on the reestablishment of the marriage of Bertrande and Martin, and, although I think that view ignores the pragmatism in my argument, I find her closer to the mark than Finlay.[99]) I exculpate a treacherous fraud by making him a pardonable self-fashioner; I believe a collusive and adulterous Bertrande "honorable"; I am out to subvert Coras's text by "accusing" him of "egregious shaping of evidence."[100]

These are Finlay's moral understandings, not mine. They come in part from his inattentiveness to the whole argument of my book and his deafness to my authorial voice. I attribute views to my characters—a sense of honor to a sixteenth-century Bertrande, a description of Martin by Coras as "the hard-hearted man with the wooden leg"[101]—and Finlay assigns them to me. On the contrary, in *The Return of Martin Guerre*, I let all of my characters have their say; I try to construct their stance toward the world so that they are understandable in terms of the range of values in their day. Martin and Pierre Guerre get much more balanced treatment at my hands than they do at the hands of Coras and Le Sueur. In their texts, Martin is only an irresponsible husband who has abandoned his family and fought for the enemies of France; he behaves abominably in court toward Bertrande and is lucky the judges decided that he deserved no punishment beyond the dishonor he had already suffered at the hands of Arnaud du Tilh. My text incorporates these judgments but also goes behind them to suggest why a sixteenth-century villager, imbued with Basque manly ideals, could reject the peasant values of property and family and prefer to seek his fortune in Spain. I stress the costs to Martin Guerre of losing his leg and of almost losing his social identity and the ambiguities in his position after he returned to Artigat.

In the texts of Coras and Le Sueur, Pierre Guerre is an unsympathetic figure, proven to have broken the law, suspected of further lying, a brutal man who puts property values above all else. When he finally emerges in the right, he is portrayed as pathetic—"this poor Pierre Guerre"—and praised only belatedly for his expenditures and risks in unmasking the impostor.[102] Here again, I use these judgments but go beyond them: the old tile-maker represents another Basque path, loyal to patrimony and land and, after an initial acceptance of the impostor,

[98] Finlay, "Refashioning of Martin Guerre," 571.
[99] Miranda Chaytor, "Tale of Two Suitors," *New Society*, 67, no. 1219 (5 January 1984): 18–19.
[100] Finlay, "Refashioning of Martin Guerre," 564, 566.
[101] Coras's description of Martin's response to the tears of Bertrande is that he "ne monstra oncques un seul signe de douleur et tristesse: ains au contraire d'une austere, et farouche contenance . . . ne daignant presque la regarder" (*Arrest Memorable*, 106). The judges in that first encounter "ne peurent oncques amolir son cueur." The word "hard-hearted" is Coras's.
[102] Coras, *Arrest Memorable*, 142, Annotation 99.

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loyal to the conventions of legitimate birth and to truth-telling about identity as a basis for ordered family life.

As for Bertrande and Arnaud, I flesh them out as historical actors, presenting their values and actions as they might have interpreted them, but then introduce other perspectives—from the sixteenth century and from today—to show the moral dilemmas and dangers posed by invention. Clandestine identity is not, I point out, the same as clandestine marriage; a possible rebirth as a Protestant would not, in the eyes of a Reformed church, justify a false rebirth as another man. Especially, at the end of the chapter on quarrels, just after the arrest of Arnaud and while Bertrande is thinking about whether she will agree to be a formal plaintiff in the case, I stop all the action and ask whether Arnaud du Tilh ever had a chance of "getting away with it."[103] This is the problem of lying, posed through the present-day inhabitants of Artigat, who said, "That pretty rascal, he lied." (My model, remember, is not the cunning Arnaud and the passive villagers, but the cunning, assisted Arnaud and villagers who "too easily believe.") My conclusion is that an *unacknowledged* lie of this magnitude, even one dependent to some extent on the hidden or open acquiescence of the sisters, of Pierre (initially), and, more distantly, of many of the villagers, would ultimately breed mistrust and disloyalty. Disloyalty within Arnaud (were these really his people?), mistrust of him by Bertrande (hadn't he initiated a relation by a trick?), mistrust of him by the village (maybe he is a magician). With Montaigne, *The Return of Martin Guerre* argues for the presence of invention everywhere in speech and culture and yet affirms, "We are human beings, and hold together only by our word."[104]

I do not, however, let Montaigne have a definitive word against Coras. They remain always in debate. Montaigne criticizes Coras's sentence from the luxury of detachment, as one who speaks "not as a judge . . . but by way of conversation," balancing other people's vehemence with the questioning of "a tumultuous and vacillating mind."[105] But Coras had to make a decision in the face of a bitterly divided family and village, and his *Arrest Memorable* humbly reveals to everyone the perplexities of a "vacillating mind." Rather than a plot to discredit that text, my analysis of the *Histoire prodigieuse* gives it a human value, shaped, "lying a little," its author implicated in its creation, yet aspiring to a true report and yielding more than its author intended.[106]

Nor did I exempt myself from this predicament, suggesting my own implication in *The Return of Martin Guerre* and the pull exerted on me by competing modes—visual and textual—for telling about the past and by competing moral positions. In my preface, I opened the gap between what I hoped to accomplish and what the sources could yield with absolute certainty; by my last page, the reader has had ample encouragement to think about other ways of making sense of the evidence.

[103] Davis, *Return of Martin Guerre*, 47, 50, 59–61.
[104] Montaigne, "Des Menteurs," in *Essais*, Book 1, chap. 9, *Oeuvres*, 37.
[105] Montaigne, "Des Boyteux," in *Essais*, 1010.
[106] Davis, *Return of Martin Guerre*, 108.

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A NUMBER OF THOSE READERS HAVE DONE SO. Their views show the possibilities in interpreting sources that Finlay considers fixed in meaning. Robert Finlay's theory is that all the Guerres and the entire village of Artigat were absolutely tricked by a treacherous Arnaud du Tilh. Bertrande is forced by Pierre Guerre to testify against Arnaud at the trial, but her deception by him lasts until she is confronted with the real Martin Guerre in the courtroom. This view has the advantage of interesting starkness and simplicity, and individual phrases can be cited from Coras in support of some of it. (Finlay cannot call on a literal phrase from Coras in support of Bertrande's last-minute enlightenment, since Coras's argument for the wife's good faith rests partly on the notion that, as soon as she perceived Arnaud was an impostor, she became a plaintiff against him. Le Sueur, too, assumes she had changed her mind when she agreed to the case.[107] For this, Finlay is going to have to admit to the work of interpretation.) To establish its plausibility, this view would have to confront all the contrary evidence of Bertrande's knowledge, Arnaud's change of behavior, Pierre's change of heart, and the court's deliberations. More important, one would want to embed the traitor and dupe in a sixteenth-century rural world. Here I foresee difficulty, especially with Bertrande. The sole trait Finlay recognizes in her is marital fidelity, and it leads her to suffer passively one piteous misfortune after another. Patient Griseldas are not impossible, to be sure, but the women I meet in the Artigat archives seem more willing to take matters into their own hands.

An opposite view of the case emerged several years ago out of a University of Iowa graduate seminar directed by Ralph Giesey, which examined the *Arrest Memorable* and *The Return of Martin Guerre* especially within the framework of Roman law.[108] The "Hawkeye Propositions" suggested that "the imposture of Arnaud was known from the outset by everyone in Artigat (or at least all who counted)." Although initiated by accident, the imposture was "perfected by collusion with Martin's relatives and friends." The Hawkeye reasoning incorporated my arguments about the advantages to the family and the village of having an heir back in place but went on to consider the economic and legal advantages to the Guerres of replacing Pierre's guardianship over the patrimony with the freer options allowed to a paterfamilias. The agreement broke down when Arnaud demanded the fruits from the property for the years of Martin's absence; Pierre tried to kill Arnaud ("his use of force betrays his original commitment to the imposture") and, when that failed, suborned Bertrande to accuse him. The Hawkeye Propositions then support my view of the trial: "Bertrande plays for a fail-safe situation."[109]

The Hawkeye Propositions of conscious complicity seem more plausible than the theory of a totally deceived family and village, and they are informed by a superb

[107] Coras, *Arrest Memorable*, 106, Annotation 78. Le Sueur, *Historia*, 9; *Histoire*, Biv^{r}.

[108] "Hawkeye Propositions on Martin Guerre," November–December 1982. Of the several excellent papers that came out of this seminar, that by Thomas A. Kiefer, "Family Law and the Case of Martin Guerre," had the most bearing on the argument in regard to complicity. I want to thank Ralph Giesey for permitting me to discuss this unpublished material in this essay.

[109] "Hawkeye Propositions on Martin Guerre," November–December 1982.

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understanding of the laws and strategies of heirship in sixteenth-century France. They certainly are appropriate for families so eager for property consolidation and alliance that they marry Bertrande and Martin as children and then, in the wake of Martin's departure, support a marriage between Pierre and Bertrande's mother, a degree prohibited by Catholic law.[110] A mixture of accident and family intervention in the staging of imposture is also found in Jeanne Domecourt's confession about the Charlotte Lebel case. Jeanne was first identified as Charlotte by a villager of Esmery, who saw her going about her affairs in Saint Quentin and said she "had goods belonging to her in Esmery." When summoned by Jeanne's canon for more details, the villager returned with Charlotte's half-brother, who gave over Charlotte's titles of property to the canon and called Jeanne "my sister."[111]

Yet one cannot tell what the half-brother knew: was he paid off by the canon, did he simply think he was seeing his sister, or something in between? I have the same misgivings about the Hawkeye Propositions. If all the important villagers were openly associated with what was going on, would not some accusation of collusion have surfaced in the trial, especially since witnesses from Artigat were bitterly divided? Even limiting the open collusion to the Guerres,[112] I find it harder to conceive of that first meeting (in terms of all we know about the case) as part of a family plan with Arnaud's shadowy informers, Pujol and Quilliet, than as a surprise encounter in which contrary signs were underplayed by the Guerres and Bertrande for the sake of having a much needed relative back. I prefer to leave our options open to a range of "knowing," from the barely conscious to the "too easily persuaded" to explicit agreement.[113]

Further, when one situates the Hawkeye Propositions in the grid of local culture, they image peasants as too exclusively concerned with calculations about property and patrimony and fail to take into account other and sometimes competing concerns about honor, progeny, legitimacy, regional custom (here, Basque values), peaceable households, affectionate marriages, and sin. They also center Pierre and Arnaud as actors—the guardian and the substitute paterfamilias—and reduce the initiative of Martin's sisters and Bertrande. But it was the sisters who were the first to see the impostor at the next village and come back for Bertrande and who were the last ones in the family to defend him as their brother, adjusting along the way

[110] Pierre was marrying the blood relative of his nephew's wife, an affine of the second degree. This would presumably require a dispensation. I am grateful to Lisa Jardine and Charles Donahue, Jr., for comments on this matter.

[111] AN, $X^{2A}98$, 32^{v}–33^{r}.

[112] Which the Hawkeye Seminar was willing to do when I raised this problem about the court testimony with them and pointed out that surely parlementary judges, used to dealing with village conspiracies, would have had some inkling of village collusion (letter to Ralph Giesey of 14 November 1982). Ralph Giesey responded, "We concede that the villagers need not have been explicitly conspirators, but rather that they let the family do as it wanted" (letter of 20 December 1982).

[113] G. W. Pigman has written an essay, presented at the annual meeting of the Modern Language Association in San Francisco in December 1987, and drawing on his training as a psychoanalyst and as a literary scholar and discussing the Martin Guerre case. He there spells out the uncertainty of our knowledge of another person's experience, not merely of another person from the distant past but even of another person in a clinical situation today.

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to a man who could no longer speak the Basque of their childhood. And it was Bertrande that the Parlement of Toulouse suspected of collusion.

Finlay's interpretation rests on the assumption that individual identity is fixed and sure but, somehow, the capacity to deceive and be deceived about identity is almost limitless. The Hawkeye Propositions rest on the assumption that people know who they are but that social identity is the important thing, and that niches can be filled by social agreement. Stephen Greenblatt, starting from *The Return of Martin Guerre* and a remarkable grasp of literary process, suggests in an important essay, "Psychoanalysis and Renaissance Culture," that to sixteenth-century observers it was uncertain what was under the mask of persona, that identity was not established by an internal sense of a past stretching back to infancy, and that what was at issue in the case was "not Martin Guerre as subject but Martin Guerre as object, the place-holder in a complex system of possessions, kinship bonds, contractual relationships, customary rights, and ethical obligations." The story of Martin Guerre is a step in the long historical process by which the idea of "proprietary rights to the self [is] secured" and psychoanalysis gains its subject.[114]

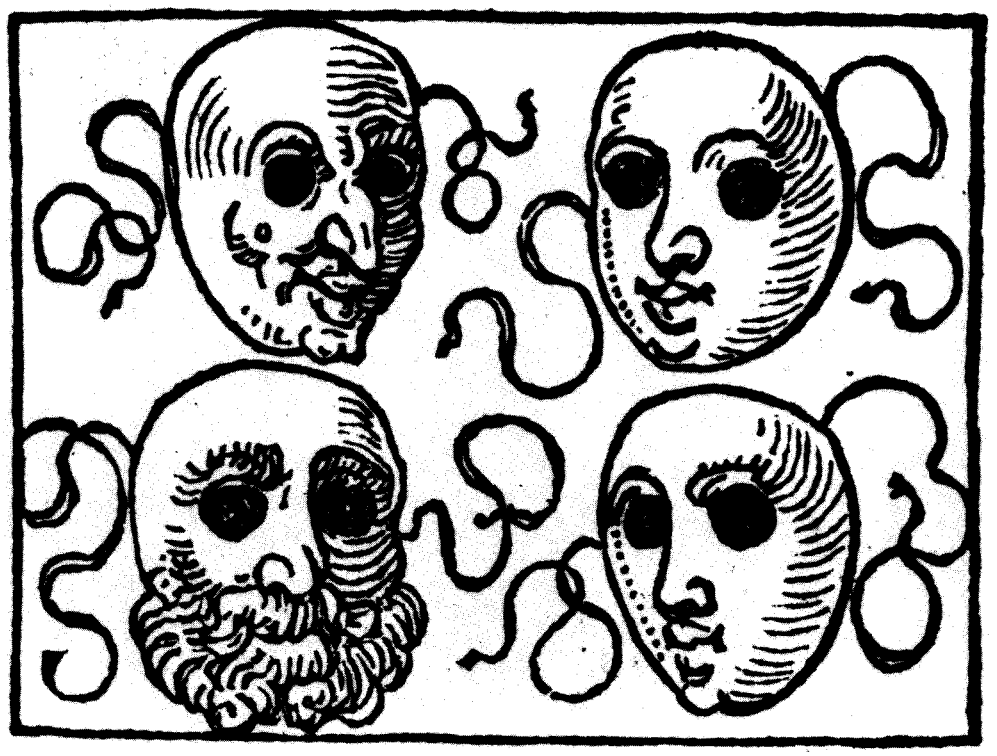
I agree with Stephen Greenblatt about the significance of external attributes, social roles, and contractual places in determining identity in the sixteenth century and in the Martin Guerre story in particular, and his essay makes us understand afresh Renaissance delight in the theatrical performance of successful imposture. But I think his argument goes too far, underestimating the ways in which people with a different sense of boundary from ours or Freud's—of property boundary and bodily boundary—understood a personal history and perceived the guarantee for some stability to the self.[115] In the Martin Guerre case, intimate memory was thought of as a personal possession. Forgetting did not count so much: that the prisoner could not speak Basque was not proof of who he was one way or another, for Martin had left Hendaye when he was about two. But the court could not see how the prisoner, if he were not Martin Guerre, could remember intimate things that had taken place only between Martin Guerre and Bertrande de Rols. Further, this memory was drawn on in one privileged relation, that between God and the individual person, mediated through the priest in Catholic confession or directly in Protestant prayer. The devil might trick one, but there was something there, given by God, to be tricked, and at the resurrection one would rise and be judged in one's own body and under one's own name. Bertrande and Arnaud's collusion on a memory test shows a recognition of the inner definition of the self, here used to sustain a position as husband that Martin had wrongly abandoned. Bertrande's display of guilt in court and Arnaud's final remorse show the sixteenth-century limits to the social invention of identity.

[114] Stephen Greenblatt, "Psychoanalysis and Renaissance Culture," in Patricia Parker and David Quint, eds., *Literary Theory/Renaissance Texts* (Baltimore, Md., 1986), 210–24.

[115] I have considered some aspects of this topic in "Boundaries and the Sense of Self in Sixteenth-Century France," in Thomas Heller, Morton Sosna, and David E. Wellbery, eds., *Reconstructing Individualism: Autonomy, Individuality, and the Self in Western Thought* (Stanford, Calif., 1986), 53–63, 332–36.

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Unless that was not really Martin Guerre who came back. Some readers have wondered about the mysterious Martin de Guerre in my notes,[116] executed in 1555 for transporting "enemy letters" from Burgos, where our Martin Guerre had been living, via Irgun and Bayonne to Spanish merchants in Rouen.[117] To such a query, I respond that the evidence about Martin Guerre at the 1557 battle of Saint Quentin is good, but, whoever he was, the man with the wooden leg won the assent of Bertrande and the Guerres and died as Martin Guerre in Artigat some thirty years later.



Masks from Guillaume de La Perrière, *Le Theatre des Bons Engins* (Lyon, 1549). Photo courtesy of Houghton Library, Harvard University.

[116] Davis, *Return of Martin Guerre*, 147 n. 2.
[117] AN, JJ263A, 271r–272r.

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Uneven Development, the Autonomy of Politics, and the Dockworkers of Nineteenth-Century Marseille

WILLIAM H. SEWELL, JR.

THE DOCKWORKERS OF MARSEILLE CONSTITUTE A HIGHLY EXCEPTIONAL CASE in the history of the nineteenth-century French working class. Between the Restoration and the Second Empire, when labor organizations were regarded as illegal "coalitions" under the penal code, Marseille's dockworkers were nevertheless openly organized in a mutual aid society that was actually a continuation of their Old Regime corporation. By means of this society, they tightly restricted entry into the trade, minutely controlled all work done on the docks, and maintained wages superior not only to those of other unskilled laborers but to those of virtually all skilled workers as well. In a working-class world populated by repressed, fragmented, and struggling labor organizations, Marseille's dockworker corporation was supremely unified and self-confident, and was tolerated, sometimes even encouraged, for some four decades by merchants and political authorities.

It is obvious that any phenomenon so singular must be explained largely by local and particular causes, and much of this article will concentrate on peculiarities of Marseille's history. But my aim is broader. I want to explain this particular case but to do so in a way that simultaneously contributes to a rethinking of nineteenth-century labor history. An exceptional case like that of Marseille's dockworkers lends itself to such a rethinking surprisingly well, for patterns once deemed exceptional by nineteenth-century labor historians have increasingly turned out to be the rule.

Before the early 1960s, labor history was dominated by a monolithic model of capitalist development. The rise of capitalism meant the growth of the factory system of production, and the growth of factories meant the expansion of a proletariat and therefore the development of radical and class-conscious labor movements. In spite of research that chipped away at the edges of this monolith,

A first sketch of this article's argument was presented at the American Historical Association meetings in San Francisco in 1978. Subsequent versions were presented as the Andrew Appleby lecture at San Diego State University in 1984, at a seminar of the Ecole des Hautes Etudes en Sciences Sociales in 1984, as a lecture at the University of Washington in 1985, and to an interdisciplinary historical sociology seminar at the University of California, Santa Cruz, in 1986. I received valuable comments at each of these occasions. The final version of the article was read and criticized by Geoff Eley, Howard Kimmeldorf, William Reddy, and Joan W. Scott. I am grateful for their advice. Part of the research on which this article is based was funded by a grant from the National Science Foundation.

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it was only with the publication of E. P. Thompson's *The Making of the English Working Class* in 1963 that it began to crumble. Since then, new research has made capitalist development seem more and more multiform. Thompson and his many followers have emphasized the penetration of capitalism into the handicrafts, by means of intensified division of labor, subcontracting, sweating, and other exploitative practices that undercut artisans' skills and their control of labor markets and workplaces.[1] Meanwhile, research on textile factory workers and coal miners has found that these supposedly paradigmatic proletarians were by no means the regimented and uniformly exploited masses posited by the classic account; they formed tightly knit, kin-based communities, they prized their skills, maintained remarkable workplace autonomy, and their relations with employers often took the form of commercial subcontracting rather than straightforward wage labor.[2] Charles Sabel and Jonathan Zeitlin have even suggested that the factory itself was a contingent rather than a necessary product of industrial capitalism, and that alternatives to mass production may yet win the day against the classic satanic mill.[3] Across the entire range of industries, nineteenth-century capitalist development turns out to have been a most untidy affair.

Perhaps the most heroic effort to catalog the diversity of the impact of capitalism on nineteenth-century workers has been Raphael Samuel's essay "Workshop of the World: Steam Power and Hand Technology in Mid-Victorian Britain."[4] This article documents in staggering detail the intimate cohabitation of new mechanical techniques, old handicraft skills, and backbreaking manual labor. In his effort to bring some conceptual order to the diversity of nineteenth-century British industrial practices, Samuel relies on Trotsky's notion of "uneven and combined development."[5] This concept was originally invented to explain the deviations of backward Russia from the supposedly orderly sequence of West European development, but Samuel transports it from the underdeveloped margins to the core of the nineteenth-century capitalist system—to the "workshop of the world."

[1] E. P. Thompson, *The Making of the English Working Class* (London, 1963), esp. chap. 8. Work on French artisans that explores this problem includes Christopher H. Johnson, "Economic Change and Artisan Discontent: The Tailors' History, 1800–48," in Roger Price, ed., *Revolution and Reaction: 1848 and the Second French Republic* (London, 1975), 87–114; Johnson, "Communism and the Working Class before Marx: The Icarian Experience," *AHR*, 76 (June 1971): 657–67; Ronald Aminzade, "The Transformation of Social Solidarities in Nineteenth-Century Toulouse," in *Consciousness and Class Experience in Nineteenth-Century Europe*, ed. John M. Merriman (New York, 1979), 85–105; Aminzade, *Class, Politics, and Early Industrial Capitalism: A Study of Mid-Nineteenth-Century Toulouse* (Albany, 1981), esp. chap. 2; Alain Cottereau's intro. to the rpt. of Denis Poulot, *Question sociale: Le Sublime, ou le travailleur comme il est en 1870, et ce qu'il peut être* (1870; rpt. edn., Paris, 1980), esp. 63–81; and Alain Faure's intro. to Agricol Perdiguier, *Mémoires d'un compagnon* (Paris, 1980). For a similar argument about nineteenth-century American workers, see Sean Wilentz, *Chants Democratic: New York City and the Rise of the American Working Class, 1788–1850* (New York, 1984).

[2] William M. Reddy, *The Rise of Market Culture: The Textile Trade and French Society, 1750–1900* (Cambridge, 1984); Royden Harrison, ed., *Independent Collier: The Coal Miner as Archetypal Proletarian Reconsidered* (London, 1978).

[3] Charles Sabel and Jonathan Zeitlin, "Historical Alternatives to Mass Production: Politics, Markets and Technology in Nineteenth-Century Industrialization," *Past and Present*, 108 (August 1985): 133–74.

[4] Raphael Samuel, "Workshop of the World: Steam Power and Hand Technology in Mid-Victorian Britain," *History Workshop*, 3 (Spring 1977): 6–72.

[5] Samuel, "Workshop of the World," 57–60.

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Unevenness, in Samuel's formulation, occurs not only in macro-relations between nations but also in micro-relations between different economic sectors within a national society, or even between different processes within the same industry.

This idea has much to recommend it. Capitalism has a unitary logic: the relentless accumulation of capital through the pursuit of profit. But, though the abstract logic of capitalist development is always the same, opportunities for the pursuit of profit vary enormously over time and space and evolve as the capitalist economy itself evolves. It is notorious that capitalist development proceeds not by uniform incremental growth and innovation in all economic sectors or industries simultaneously but by industry-specific spurts: booms in cotton textiles, or shipping, or railroad development, or automobile manufacture, or microelectronics. Even within the booming industries, innovation or dynamism rarely affects all processes at once: the timing of innovation was different in cotton spinning than in cotton weaving, in manufacture of locomotives than in manufacture of sleeping cars. One of the consequences of this unevenness is a species of combined development in the labor force. Growth in the dynamic sector often creates new, highly mechanized, classically "proletarian" industrial specialties, but it also inevitably stimulates employment in less technologically advanced auxiliary sectors, either preexisting or new. The rise of factory spinning multiplies the numbers of both hand-loom weavers and field slaves; the development of the locomotive calls forth hordes of navvies; automobile factories give rise to repair shops, taxi drivers, and filling stations; computers create keypunch operators and programmers. In nineteenth-century conditions, the establishment of any kind of factory industry led to a multiplication of handicraft workers: masons, carpenters, and joiners to build the factories; builders, tailors, shoemakers, and bakers to house, clothe, and feed their workers. The most advanced techniques developed hand in hand with the most archaic.

Samuel was concerned above all to demonstrate that the advance of machinery in mid-nineteenth-century Britain also meant a concomitant advance in physical toil. But in this respect Samuel's article is one-sided; uneven development also created (and continues to create) significant pockets of privilege in the working class. The famous "golden age of the handloom weavers" is an early and obvious example. Weavers had high wages and enviable working conditions in the era between the development of machine spinning and the adoption of the power loom. The flood of cheap machine-spun yarns caused a sharp decline in finished cloth prices, which caused a prodigious expansion in the demand for cloth, which in turn caused a scarcity of the as yet irreplaceable labor of weavers. Their earnings consequently stayed high until the power loom destroyed their temporary advantage. The case of the handloom weavers has been recreated again and again in the history of capitalism, with infinite variations, from the mule-spinners of mid-nineteenth-century Lancashire, to the skilled engineers of the nineteenth-century machine industry, to the computer programmers of Silicon Valley. Such pockets of privilege are not permanent; they may last for a few years or for several decades, but the unpredictable lurches of capitalist development eventually wipe them out. However, the same lurches that destroy privileged job sectors also create

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new ones. Although the net effect of uneven development is far from directionless,[6] neither does it result, as Samuel's account seems to imply, in a relentless increase in toil. Instead, its effect on the labor force is a richly varied, continually changing, kaleidoscopic mixture of exploitation and privilege.

The history of Marseille's dockworkers in the nineteenth century illustrates with particular clarity how uneven development could create, and in time destroy, a special category of workers. From 1815 to the 1850s, booming maritime capitalism, combined with unchanging technical and organizational conditions on the docks, raised the dockworkers from obscurity to a privileged position unique not only in Marseille but possibly in all of France. Then, in the course of a few years, a capitalist reorganization of dock work destroyed the dockworkers' niche, reducing them to little more than unskilled laborers. Although the position of Marseille's dockworkers was unusual, their story traces out a broad pattern that has been repeated over and over during the history of capitalism.

The case of Marseille's dockworkers can also contribute to the growing recognition in contemporary historical studies of the autonomy of politics. The classic conception of capitalism as the advance of the factory was always linked to a reductionist notion of the relation between economics and politics. It therefore makes sense that recognition of uneven development should be paralleled by a recognition of the autonomy (or relative autonomy) of politics. Yet the vast literature on artisans, in which the complexity and unevenness of nineteenth-century capitalism was initially documented, has been dominated by a thoroughly reductionist explanatory strategy, an effort to explain workers' politics by their economic experiences.

If artisans were radical, this line of argument goes, it was because their trades, no less than the factory trades, were being penetrated and proletarianized by capitalism, albeit in a subtler fashion. Such case studies as Christopher Johnson's illuminating and seminal article on French tailors in the first half of the nineteenth century have shown that the piecemeal proletarianization undergone by artisans could be economically devastating and have powerfully radicalizing consequences.[7] Moreover, varying forms of capitalist penetration—increasing division of labor, substitution of unskilled for skilled workers in certain phases of the production process, the development of urban putting-out networks or sweatshops, exploitative forms of subcontracting, and so on—have by now been documented for a wide variety of trades.[8] This widespread advance of capitalist penetration into the artisan trades in the nineteenth century seems to parallel the widespread advance of artisan radicalism.

But, if capitalist penetration was widespread, it was certainly not universal, nor was it uniform in its effects on the working population. For such penetration to serve as a sufficient explanation of artisan radicalization, it must be shown that

[6] Over the long run, capitalist development results in accumulation and concentration of capital, increasing separation of producers from ownership of the means of production, and commoditization of ever-widening areas of life.

[7] Johnson, "Economic Change and Artisan Discontent."

[8] See Reddy, *Rise of Market Culture*; Harrison, *Independent Collier*.

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radical artisans were drawn disproportionately from those specific trades being degraded by capitalist penetration. As far as I know, this has not been demonstrated. Indeed, the contrary can be shown for the case of Marseille in 1848, where politically radical workers were drawn not only from degraded trades but also from trades that had not been much affected by capitalist penetration and even from trades that had been privileged by capitalist development.[9] The highly uneven effects of capitalist development in the nineteenth century probably created no path of working-class economic experience sufficiently general to account adequately for the broad artisan revolts that occurred.

If this is true, explaining working-class radicalism will require a close attention to the autonomous dynamics of political change itself. Besides asking what socioeconomic experiences predisposed workers to radical politics, historians must also ask what sorts of political processes, events, and ideologies induced workers to participate in radical political movements.[10] Once again, Marseille's dockworkers serve as an illuminating case, perhaps as a sort of counterexample to Christopher Johnson's tailors. Whereas Johnson's tailors epitomize artisans driven to revolutionary politics by a tragic degradation of their trade, Marseille's dockworkers represent a trade attracted to revolutionary politics in spite of their extraordinary privilege and prosperity. Their history indicates that political processes may produce a broad-based radical working-class movement even when the unevenness of capitalist development fails to produce an economically homogeneous proletariat.

MARSEILLE'S DOCKWORKERS (*portefaix*) WERE A SUFFICIENTLY EXTRAORDINARY CASE that they were much remarked on by local authorities and other observers, especially during their "golden age" in the 1840s and 1850s. It is therefore easier to write their history than those of most nineteenth-century working-class trades. Nevertheless, the documentation, however ample for some purposes, is scarce for

[9] The artisan trades most severely affected by capitalist penetration in Marseille in the first half of the nineteenth century were the tailors and the shoemakers. Both had rates of participation in the revolutionary movement that were somewhat above the average of Marseille's working-class occupations. But their radicalism was considerably surpassed by that of bakers, housepainters, and stonecutters, whose trades do not seem to have been importantly affected by capitalist penetration, and by machinists, whose scarce and avidly sought-for skills made them among the most privileged workers in the city. These conclusions are drawn from quantitative data on persons arrested for participating in the insurrection that took place in Marseille in June, 1848, and on those rounded up as dangerous revolutionaries after Louis Napoleon's coup d'etat in December, 1851 (Archives Départementales des Bouches-du-Rhône: M6/137, M6/100, hereafter, ADBdR). Preliminary analyses of these data will be found in William H. Sewell, Jr., "La Classe ouvrière de Marseille sous la Seconde République: Structure sociale et comportement politique," *Le Movement social*, 76 (July–September 1971): 27–65; and Sewell, "Social Change and the Rise of Working-Class Politics in Nineteenth-Century Marseille," *Past and Present*, 65 (November 1974): 75–109. The first of these articles has been published in English as "The Working Class of Marseille under the Second Republic: Social Structure and Political Behavior," in *Workers in the Industrial Revolution: Recent Studies of Labor in the United States and Europe*, eds. Peter N. Stearns and Daniel J. Walkowitz (New Brunswick, N.J., 1974), 75–116.

[10] The socioeconomic experiences that can predispose workers to political radicalism need not be either classical or modified versions of proletarianization. Migration, ethnicity, neighborhood communities, cultural differences, or gender identities may, in a given case, be more important than details of the relation to the means of production.

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others.[11] This article, like most "history from the bottom up," will therefore contain a good deal of hypothetical reconstruction based on scattered, diverse, and fragmentary evidence. Given the incompleteness of the evidence, it seems best to begin with a portrait of the dockworkers in the 1840s and 1850s, which can be based on relatively complete documentation, and work backward into the earlier and more obscure decades.

Perhaps the most obvious indication of the dockworkers' privileged position at mid-century was their wage level. Estimates of dockworkers' earnings dating from the 1840s range from four to five-and-a-half francs a day. Dockworkers were actually paid on a piecework basis, so the earnings might in fact vary considerably around these sums. Because work on the docks was subject to periodic unemployment, these figures cannot simply be multiplied by the standard six-day week to derive an average weekly wage. But, even taking unemployment into account, dockworkers were among the best-paid workers in the city. Their earnings were at least twice those of other men engaged in heavy manual labor (for example, ditchdiggers or masons' laborers) and were well above those of most artisans, who usually received between three and three-and-a-half francs a day. In fact, dockworkers' wages were matched only by such highly skilled workers as glass blowers, shipwrights, machinists, watchmakers, and printers.[12] These high wages were maintained in part by strict limitations on entry into dockworking. It was very difficult to become a dockworker unless one's father already practiced the trade. Among dockworkers who were married in 1846 or 1851, no fewer than 70 percent were dockworkers' sons. This was by far the highest rate of inheritance of any occupation in the city, working-class or bourgeois; the next five occupations were fishermen with 58 percent, ropemakers with 47, tilemakers with 46, tanners with 43, and wholesale merchants (the top bourgeois occupation) with 42. The average rate for all occupations was only 23 percent.[13] These figures make it clear that the

[11] The dispute in the late 1850s and early 1860s that eventually destroyed the dockworkers' privileged position generated tracts and police files now kept in the ADBdR. The dockworkers' society was also studied in this period by two social investigators: Armand Audiganne, *Les Populations ouvrières et les industries de la France*, 2d edn., 2 vols. (Paris, 1860), 2: 265–68; and Emile Laurent, *Le Pauperisme et les associations de prévoyance: Nouvelles études sur les sociétés de secours mutuels, histoire—économie politique—administration*, 2d edn., 2 vols. (Paris, 1865), 2: 547–52. For earlier decades, the evidence is sparser but still better than for most other trades. Particularly useful are the papers of the mayor's office from the Restoration period (1814–30), which are preserved in series I of the Archives de la Ville de Marseille (hereafter, AVM). An excellent study of the dockworkers is Victor Nguyen, "Crise et vie des portefaix marseillais, 1814–1914" (Diplôme d'Etudes Supérieure, Faculté des Lettres et des Sciences Humaines, Aix-en-Provence, 1961). Unfortunately, only the final portion of this diplôme, less interesting for my purposes than the earlier portions, has been published: Victor Nguyen, "Les Portefaix marseillais: Crise et déclin, survivances," *Provence historique*, 12 (1962): 363–97. Although I differ with Nguyen on a number of points, I have learned a great deal from his work, as my footnotes in this article will testify.

[12] Wage figures for all these trades are available in *Travaux de la société de statistique de Marseille*, 4 (1840): 52–53; 5 (1841): 346–47; and 9 (1844): 72–73; and in "Enquête sur le travail agricole et industriel," Archives Nationales: C 947.

[13] These figures are based on a computer-aided analysis of Marseille's marriage registers from 1821–22, 1846, 1851, and 1869 (AVM, series 201 E). The marriage registers indicate the occupations of both bridegrooms and their fathers. For a thorough discussion of these data, see William H. Sewell, Jr., *Structure and Mobility: The Men and Women of Marseille, 1820–1870* (Cambridge, 1980), esp. 317–19. This quantitative research was supported by a National Science Foundation Grant SOC 72–05249–A01.

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dockworkers' lucrative trade was a kind of hereditary possession for their families.

The dockworkers' high earnings and high level of occupational inheritance would have been impossible without a powerful labor organization. The dockworkers were organized openly and unabashedly. Their "Society of Saint Peter and Saint Paul and of Our Lady of Mercy" was authorized by local officials in 1817, shortly after the defeat of Napoleon and the restoration of the Bourbon monarchy. Officially a mutual benefit society, it was actually a reconstitution of the dockworkers' corporation, or guild, from the Old Regime. The corporation dated back at least to the end of the fourteenth century, but its statutes were first written down in 1704. These had been amended in 1789, just before the French Revolution; the statutes of 1814, in turn, were an amended version of those of 1789.[14] In 1814, members of the old corporation and their sons could join the mutual aid society by paying a nominal fee of eight francs, while all other applicants had to pay eighty francs, a sum equivalent to better than a month's wages for most manual workers. These entry fees were the same as in the Old Regime corporation.[15] In 1841, when the society's statutes were revised again, the entry fee charged for non-dockworkers' sons was increased to the impossible sum of a thousand francs.[16] Besides erecting these high financial barriers to admission, the dockworkers' society could simply refuse membership to men who attempted to enter, even if they could pay the fee.

Restrictions on entry into the dockworkers' society also meant restrictions on entry into the trade, for the society maintained a monopoly over the loading and unloading of ships in Marseille's harbor. Under the Old Regime, members of the corporation had the exclusive privilege of carrying a special dockworkers' sack, and any man who worked on the docks without this mark of membership could be fined one hundred *livres*. This practice lost its legal standing after the revolution, but it continued to be strictly observed. In fact, the 1817 statutes of the dockworkers' society included as one of its regulations that "only dockworkers admitted into the society have the right to carry the sack."[17] Until the 1860s, it was impossible to work on the docks of Marseille without being a member of the dockworkers' society and carrying the identifying sack.[18]

The Society of Saint Peter and Saint Paul and of Our Lady of Mercy had a much more elaborate formal organization than most nineteenth-century mutual aid societies, more elaborate, in fact, than many eighteenth-century guilds. It had a Grand Conseil of sixty members that set general policy, and a Petit Conseil of twelve that managed its day-to-day business. The Petit Conseil was responsible, for example, for organizing celebrations on the festival day of Saints Peter and Paul, with masses, processions, and acts of charity. The Petit Conseil was composed of

[14] Josette Zanzi, "Les Portefaix marseillais à la fin de l'ancien régime et sous la Révolution française" (Mémoire de maîtrise, Faculté des Lettres d'Aix-en-Provence, 1969), 4–7; Laurent, *Le Pauperisme*, 2: 458–59. The statutes of 1789 and 1817 are available in AVM: 1 I 1/35, 1370.

[15] AVM: 1 I 1/35, 1370.

[16] Laurent, *Le Pauperisme*, 2: 549–50; and Nguyen, "Crise et vie," 15. A thousand francs amounted to an entire year's earnings for a skilled worker.

[17] AVM: 1 I 1/35, 1370.

[18] Laurent, *Le Pauperisme*, 2: 548–49.

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six "visitors of the sick" and six "priors" (*prieurs*). The visitors of the sick were charged with administering the society's generous benefits, which amounted to six francs a week plus doctors' fees and medications in cases of sickness and five francs a week in retirement benefits for men who had worked on the docks for thirty years or more. To finance these benefits, the society required its members to pay 3 percent of their earnings as dues.[19] By 1852, the society had holdings of 232,666 francs, placed in various banks and in municipal bonds.[20]

The priors were responsible for all affairs of the society connected with work on the docks: mediating disputes between teams of dockworkers or between dockworkers and merchants, making sure that teams were not competing with one another so as to drive down wages, and above all ensuring that only members of the society were employed on the waterfront.[21] The priors also administered an institution known as "La Muse." Dockworkers who were not members of a regular team, or whose team had no job on a given day, would report to the Muse, where they were placed on a register in order of appearance, to be given daily work as it became available. Any job not being handled by a regular team had to be channeled through the Muse, and the Muse always handled certain types of unloading—salt and grains, for example. By means of the Muse, the dockworkers' society was able to apportion equitably the available work and to cushion its members against unemployment.[22] Finally, the priors were responsible for overseeing the nature and techniques of work. The society was resolutely opposed to any technical innovations, and in 1853 it adopted a regulation formally banning all wheeled vehicles from the docks, prescribing that all burdens be carried on the dockworkers' backs.[23] The society not only had a monopoly of work on the docks, it also determined how the work was to be done and, through its priors, enforced its own regulations. Its control of work was virtually absolute.

In the 1840s and 1850s, then, the dockworkers enjoyed an enviable position. Not only were they extremely well paid but they had unmatched job security, a workable pension plan, and an equitable system of allocating work during periods of unemployment. Their elaborately organized society was tolerated by the authorities, and by means of their society the dockworkers themselves controlled the organization and pace of their work, kept interlopers off the docks, and saw to it that good wages and working conditions were maintained. Moreover, dockworkers knew they could pass all these advantages on to their sons. By comparison with almost any other workers in Marseille, or in France for that matter, the dockworkers had a very comfortable situation.[24]

[19] These are the provisions in the statutes of 1817. They were modified slightly in 1853. The 1853 statutes, which were printed together with a reproduction of the statutes of 1817, are available in ADBdR: XIV M 25/1.

[20] Nguyen, "Crise et vie," 14–15.

[21] AVM: 1 I 1/35, 1370; ADBdR: XIV M 25/1; Nguyen, "Crise et vie," 14–15.

[22] AVM: 1 I 1/35, 1370; ADBdR: XIV M 25/1; Nguyen, "Crise et vie," 16; Laurent, *Le Pauperisme*, 2: 549.

[23] ADBdR: XIV M 25/1.

[24] For a discussion of French labor organizations in the first half of the nineteenth century, see William H. Sewell, Jr., *Work and Revolution in France: The Language of Labor from the Old Regime to 1848* (Cambridge, 1980), chap. 8.

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THE PRIVILEGES OF THE DOCKWORKERS' SOCIETY WERE FIRMLY ESTABLISHED in the first years of the Restoration. But these privileges did not guarantee prosperity immediately; they became genuinely lucrative only when the long capitalist boom of the second quarter of the nineteenth century quickened the activity of Marseille's port. The dockworkers' society was established by a provisional decree of the Marquis d'Albertas, the prefect of the Bouches-du-Rhône, in 1814, only a few months after the restoration of the Bourbon monarchy. The statutes of the society were then officially approved in 1817 by the Marquis de Montgrand, mayor of Marseille. Existing documents do not make entirely clear the reasons for d'Albertas's and de Montgrand's support of the dockworkers. It was, of course, far from rare for officials of the Restoration to have sympathy for Old Regime guilds, which were viewed by many conservatives as a means of maintaining deference and discipline among the lower orders. But the Restoration's official policy was to maintain economically liberal Revolutionary and Napoleonic legislation, and no wholesale reestablishment of the guilds was undertaken. No other trade in Marseille was treated like the dockworkers, and few, if any, were so treated elsewhere in France. From both a national and a local perspective, the dockworkers' society was an anomaly. Official sanction for its statutes would have been impossible to obtain had not the authorities regarded the dockworkers' and their association as somehow exceptional.

In fact, when de Montgrand proposed official approval of the dockworkers' statutes in 1817, the Comte de Villeneuve-Bargement, d'Albertas's successor as prefect, questioned their legality. De Montgrand, who was the dockworkers' principal patron from 1814 until he resigned from office after the July Revolution in 1830, managed to overcome the prefect's misgivings. In a letter of 29 September 1817, he claimed that even under the Old Regime the dockworkers' association had not had "the character of a privileged corporation"; its statutes had been "purely the result of measures of public interest decreed . . . for the maintenance of an order necessary among the men of that profession." These reasons were as valid in 1817 as they had been before the revolution; the society's regulations would maintain "a discipline extremely favorable to tranquillity, to public order, to the observation of principles of honesty and fidelity indispensable to the interests of commerce." Moreover, de Montgrand argued, regulations analogous to the proposed statutes of Marseille's dockworkers existed for work "on the quays and in the ports" of cities all over France. He even procured a copy of the recently enacted statutes of the dockworkers of Nantes to illustrate this argument.[25] Finally, de Montgrand argued that approving the statutes would have the additional benefit of assuring the dockworkers' loyalty to the new monarchical regime. "I will not speak, Monsieur le Comte, of the essential advantage there is for authority,

[25] AVM: 1 I 1/35. In fact, the statutes regulating the Nantes dockworkers established no dockworkers' society with the power to limit entry into the occupation or to police the docks, and the mayor of Nantes prefaced the regulations governing dock work with this statement: "[I]t is not possible, according to the principles of current laws, to accord to [the dockworkers] an exclusive privilege for the work in question." The Nantes statutes actually indicate how extraordinary the Marseille dockworkers' association was.

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from the political point of view, of keeping under a regular dependence, by its direct action on the heads of the association, a so numerous mass of men whose lapses or movements could in many circumstances be very disquieting."[26]

But this political advantage was incidental to the central point. Dock work, de Montgrand implied, had a special public character that distinguished it from other trades, and it was therefore acceptable for the municipality to approve regulations that ensured its orderly performance. Rather than working in enclosed, private spaces subject to the private discipline of the proprietor or entrepreneur, dockworkers worked outdoors, in a public space controlled by the municipality. The regulation of dock work, therefore, could be seen as a simple exercise of the municipality's police authority, which in this case was to be largely delegated to the dockworkers' organization itself. The dockworkers' society, then, was a kind of semi-public institution. As such, it was expected, under Old Regime and new, to perform certain public functions for the municipality. The society was required by its statutes, both in 1789 and in 1817, to constitute itself as an emergency fire brigade at the command of the municipality.[27] During the eighteenth century, the dockworkers' corporation had frequently been called in by the municipality to help maintain order, for example, in 1777 during an official visit of Louis XVI's brother, in 1781 to guard the door of the cathedral during a mass celebrating the birth of the Dauphin, in 1784 during a balloon launching, and in 1789 after the pillaging of the house of a tax farmer.[28]

In any case, the dockworkers' society was treated very differently from associations organized by other groups of workers—whose attempts to limit entry into the trade, to control the nature and pace of work, or to maintain wage levels had to be carried on in secret. Mayor de Montgrand approved the charter of the dockworkers' society, defended it against the prefect, and was deeply involved in the day-to-day administration of its affairs—intervening in disputes about work, fines, and expulsions.[29] During his tenure, de Montgrand seems to have regarded the dockworkers' society almost as an auxiliary branch of the municipal government, one subject to his continual and benevolent oversight.

This persistent support from the mayor and the municipality enabled the dockworkers' society to establish itself firmly on the waterfront. Institutionally, the dockworkers' society seems to have functioned as effectively in the 1820s as it later did during the dockworkers' golden age in the 1840s and 1850s. Although dockworkers seem to have been involved in more disputes with other transport workers in the 1810s and 1820s than in succeeding decades, they successfully maintained their monopoly against all challenges. No satisfactory figures on wages are available for the early decades, but what evidence there is indicates that the piece-rates paid to dockworkers were essentially the same in the late 1810s as at

[26] Letter from mayor to prefect, 29 September 1817, AVM: 1 I 1/35.
[27] ADBdR: XIV M 25/1; AVM: 1 I 1/35.
[28] Zanzi, "Les Portefaix marseillais," 39–40.
[29] See the mayor's correspondence in AVM: 1 I 1/36.

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mid-century.[30] The marriage registers of the early 1820s show that the occupation of dockworker was passed from father to son at essentially the same high rate in the 1820s as at mid-century. Dockworkers who were married in 1821 or 1822 were sons of dockworkers in 73 percent of the cases—once again the highest rate of inheritance of any occupation in the city. In all these respects, the dockworkers' control over their trade seems to have been as solid in the 1820s as it was in the 1840s or 1850s.

Even with all these advantages, dockworkers seem to have been far less prosperous in the late 1810s and the 1820s than they became in subsequent decades. During the French Revolution and the Napoleonic period, dockworkers had been decimated by the effects of maritime war and blockade, which had cut the traffic of the port to a trickle. Even after the revival of maritime commerce in the 1820s, their situation was by no means splendid. It was only the long capitalist boom of the French and European economies during the second quarter of the nineteenth century that made possible the prosperity of the 1840s and 1850s. Between 1821 and 1851, the population of Marseille nearly doubled, and the traffic of the port more than doubled, with the precise figure varying according to the method of estimation.[31] This prodigious growth in port traffic is traced in Figure 1. The spectacular rise before the crisis of the late 1840s was a consequence of several factors: an absence of general European wars, an expansion of world trade, the development of industry in Marseille, in France, and in the rest of Europe, and improvements in communications from Marseille to the inland of France.[32]

One would expect this rapid increase in the activity of the port to have brought about an equivalent rise in the number of dockworkers. But in fact the number of dockworkers in Marseille appears to have been virtually constant. There are no really satisfactory figures for the number of dockworkers employed in the city before the middle of the nineteenth century, but a rough estimate of changes in the size of the labor force can be derived from figures for the number of dockworkers who married in a given year.[33] This figure averaged thirty-nine per

[30] Nguyen found fragments of piece-rate schedules for various grains, which indicate that rates had risen by no more than 10 percent between 1818 and 1853. "Crise et vie," 28.

[31] The population of Marseille increased from 109,483 in 1821 to 195,135 in 1851. Sewell, *Structure and Mobility*, 147. The amount of customs duties paid in the port of Marseille rose by 178 percent from 1817–21 to 1847–51; the carrying capacity of ships entering the port rose by 121 percent from 1825–29 (the first years for which these figures are available) to 1847–51. The choice of different starting and ending points for such comparisons would result in somewhat different figures, but nearly all estimates indicate something between a doubling and tripling of maritime commerce from the early 1820s to the late 1840s. The figures are derived from Jules Julliany, *Essai sur le commerce de Marseille*, 2d rev. edn. (Marseille, 1842), 1: 145, 162; Casimir Bousquet and Tony Sapet, *Etude sur la navigation, le commerce et l'industrie de Marseille, pendant la période quinquennale de 1850 à 1854* (Marseille, 1857), 25–29; and *Travaux de la société de statistique de Marseille*, 1 (1837): 70; and 19 (1855): 92.

[32] For a fuller discussion of the growth of maritime commerce, see Sewell, *Structure and Mobility*, 18–23.

[33] The normally relatively accurate "Enquête sur le travail agricole et industriel" estimates (Archives Nationales: C 947) put the number of dockworkers at mid-century at 2,500, whereas the far more accurate figures derived from a sample of every tenth household in the census of 1851 indicate only 1,530. This remarkable overestimate, which I suspect was supplied to the Chamber of Commerce's investigators by the dockworkers' society, should make one wary of accepting any contemporary estimates not based on solid figures of some kind. There are, unfortunately, no census figures giving

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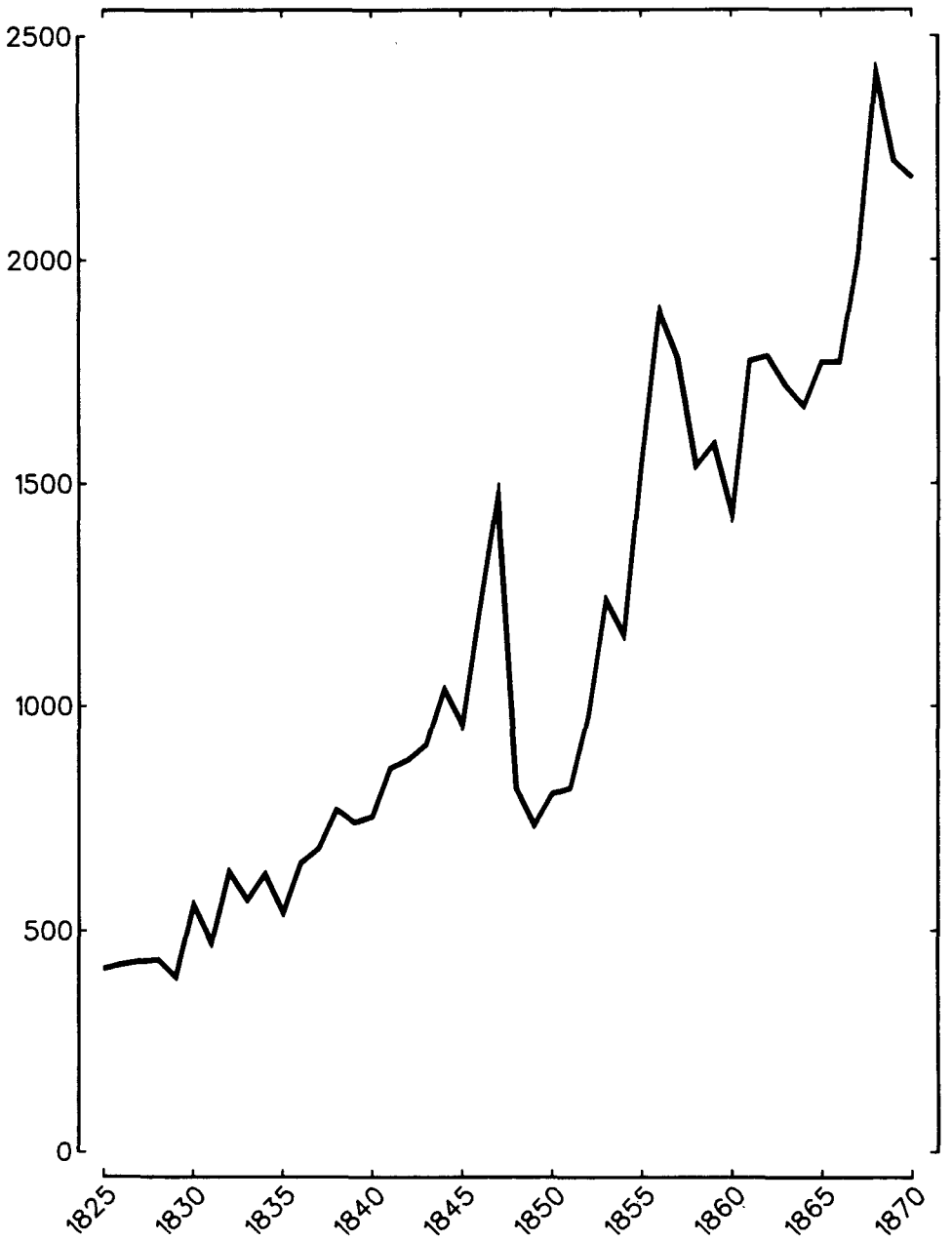


FIGURE 1: Carrying capacity of vessels entering the port of Marseille, 1825–1870, in thousand tons. Adapted from William H. Sewell, Jr., *Structure and Mobility: The Men and Women of Marseille, 1820–1870* (Cambridge, 1985), 20.

occupation before 1851. Under these circumstances, figures from the marriage registers seem the best alternative. The number of men in a given occupation who were married depends on a number of factors besides the total number employed in the occupation, such as the age structure of the trade, changes in the age at marriage or the rate of marriage, and so on. Nevertheless, this statistic is probably accurate enough to provide a rough estimate of the magnitude of change in the size of an occupation over time.

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year in 1821 and 1822, and remained essentially unchanged, at an average of thirty-seven per year, in 1846 and 1851. Thus, in a period when the amount of labor to be done approximately doubled, the number of dockworkers available to do it remained essentially constant. These figures seem to indicate an enormous change in the market for dockworkers' labor, one that, according to conventional economic theory, should have resulted in a steep rise in wage rates. Yet the evidence, which admittedly is thin, indicates no important rise in the piece-rates paid to dockworkers in this period. What the available evidence suggests is a quite different development but one no less favorable to the dockworkers.

By all reports, Marseille's dockworkers were able to handle the volume of goods that entered the port in the 1840s without relaxing either restrictions on entry into their society or control over work on the docks. This implies that there must have been a good deal of slack twenty or thirty years earlier, when the same number of men were handling only half as much work. If unemployment and underemployment was common in the 1820s and early 1830s, the main effect of the commercial boom on Marseille's dockworkers may not have been to increase wage rates but rather to increase the level of employment—by increasing either hours of labor, intensity of labor, or both. Since dockworkers were paid by fixed piece-rates, the result of any of these increases in employment should have been a proportional rise in their earnings. Evidence bearing on this question is extremely scarce, but most of it is compatible with the hypothesis of a constant labor force whose level of employment rose substantially over time.

The first piece of evidence—hardly conclusive—is literary in nature. It derives from the memoirs of Victor Gelu, a writer and singer of Provençal songs, who was born in a popular quarter of Marseille in 1806. As a young man in the middle 1820s, Gelu spent much of his leisure time with young worker friends. Dockworkers were especially prominent among his drinking companions. When he was looking for entertainment, Gelu would stroll down to his neighborhood barbershop, which had a back room where young men gathered to talk, drink, and gamble. There he was always sure of finding plenty of dockworkers temporarily out of work; according to Gelu, this particular barbershop had so many dockworkers that it "had become almost a branch office of the Muse."[34] From Gelu's account, it would appear that days of unemployment were a normal experience of dockworkers, or at least of young dockworkers, in the 1820s.

The second piece of evidence—suggestive but not much more conclusive than the first—is the pattern of contestation on the docks. The period of the largest number of disputes seems to have been the late 1810s and the early 1820s, and, from what little we know of these disputes, they usually involved the problem of unemployment in some way. Thus, in 1819 and 1821, the dockworkers protested that sailors were being used to unload ships, thereby depriving them of work; in 1824, dockworkers who were registered at the Muse complained to the priors that some of the masters were bypassing the Muse and taking on new men of their own

[34] Victor Gelu, *Marseille au XIX^e siècle*, eds. Lucien Gaillard and Jorgi Reboul, intro. by Pierre Guiral (Paris, 1971), 143.

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choice. Throughout the period, there were troubles between dockworkers and *robeirols*—laborers who waited around on street corners, ready to undertake assorted carrying tasks.[35] In principle, these *robeirols* were easily distinguishable from dockworkers by the baskets they wore on their backs for carrying their loads.[36] This pattern of frequent disputes involving issues of employment in the 1820s followed by a virtual absence of disputes in the 1830s and 1840s fits the hypothesis of initially high levels of unemployment, followed by increasingly full employment in the 1830s and 1840s. One might have expected a rather different pattern had the rise in demand for dockworkers' labor been met mainly by changes in wage rates. In such circumstances, one might have expected relative quiescence in the early period, when high unemployment rates would have put workers in a weak bargaining position, and increasing contestation over wage levels as the workers took advantage of their increasing relative scarcity to force a raise in their rates.

The third piece of evidence—in my opinion, somewhat more conclusive than the other two—concerns changes in the number of workers employed in other heavy transportation trades. Although the number of dockworkers appearing on the marriage registers remained constant, the number of men identifying themselves as "carters" or "loaders" rose very sharply, from only fifteen per year in the early 1820s to fifty-three per year at mid-century. If these carters and loaders are added to the thirty-nine and then thirty-seven dockworkers, the rise in transportation workers as a whole was from fifty-four in the early 1820s to ninety at mid-century, not as steep as the rise in the traffic of the port but not far behind (see Table 1). These figures seem to indicate a major change in the division of labor between the privileged dockworkers and the distinctly unprivileged loaders and carters.[37]

I have been able to find no direct evidence about division of labor in the transportation trades. However, two hypotheses suggest themselves, one concerning changes in the work done by dockworkers and the other concerning changes in the work done by carters and loaders. According to the first hypothesis, in the 1820s, dockworkers may have been supplementing the earnings they gained from loading and unloading ships by engaging in assorted other carrying and hauling jobs that they picked up on a day-labor basis. Hence, as the traffic of the port rose in the 1830s and 1840s, they would have concentrated their efforts exclusively on the high-paying work of loading ships, leaving less lucrative carrying work to loaders and carters. The second hypothesis would suggest that dockworkers did not engage in other transportation labor even in the 1820s but that, as the volume of goods to be unloaded rose in the later 1830s and 1840s, they found ways to employ carters and loaders as auxiliary workers on the docks under the close

[35] Nguyen, "Crise et vie," 19–22; AVM: 1 I 1/36.

[36] This requirement that *robeirols* wear the *pallier* was written into the statutes of the dockworkers' society in 1817. ADBdR: XIV M 25/1.

[37] The dockworkers and the carters and loaders were from distinct social categories. This difference can be seen clearly in their recruitment patterns. Only 37 percent of the carters and loaders who married in Marseille in 1846 and 1851 had been born in Marseille, as against 89 percent of the dockworkers. Forty-one percent of the carters and loaders were sons of agriculturalists and 29 percent sons of unskilled workers.

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TABLE 1
Number of Transport Workers Marrying

	1821–22	*1846–51*	*1869*	*increase 1821–22 to 1846–51*	*increase 1846–51 to 1869*
Dockworkers marrying per year	39	37	49	−6%	+32%
Other transport workers marrying per year	15	53	78	+253%	+47%
Total	54	90	127	+67%	+41%

supervision and control of the dockworkers' society, perhaps in moving goods to warehouses or to different locations on the docks once they had been unloaded from ships by dockworkers. This arrangement would have made it possible to utilize the cheap labor of carters and loaders without giving up the dockworkers' monopoly on cargo handling. The fact that the dockworkers' society was moved to ban the use of wheeled vehicles on the docks in 1853 suggests that some such experiments must have been going on. Either of these changes, or a combination of the two, would be consistent with the quantitative data. In any case, it seems clear that, as the traffic of the port increased, the dockworkers drew the practical boundary separating their work from that of other transportation laborers more tightly around the actual loading and unloading of ships.

The available evidence suggests a marked improvement in the dockworkers' real conditions between 1830 and 1848, in spite of the surface continuity of institutions. The dockworkers' society was no less elaborate in the 1820s than it became by mid-century, its monopoly of work on the docks was no less complete, and men were every bit as capable of passing their occupations on to their sons. But these privileges (and privileges they were in the 1820s) were far less lucrative than they became by the 1840s. Unemployment and underemployment were endemic. Wages were good as long as work was available, but days without work were all too common. Hence dockworkers spent many daylight hours drinking and playing cards in the local bistros and cafes and may also have taken low-paying jobs doing assorted hauling and carrying to make ends meet. In a time when work on the docks was scarce, conflicts frequently took place about its allocation among dockworkers, between dockworkers and sailors, or between dockworkers and *robeirols*. Dockworkers were by no means desperate or impoverished in the 1820s or early 1830s; their wages were good, and income could be supplemented by occasional day labor. But they were by no means so prosperous as they became by mid-century.

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By the 1840s, some prominent Marseillais had begun to denounce the dockworkers' society for its restrictive practices. The most notable was Jules Julliany, *premier adjoint* to the mayor under the July Monarchy, and the author of a vast compendium on the commerce of the city. Writing in the early 1840s, he proposed that the city's merchants act to rid themselves of "this monopoly [that has] always weighed on the commerce of Marseille" by simply ignoring the regulations of the dockworkers' society and hiring other workers of their choice. This action, Julliany implied, would probably be met by violent resistance from the dockworkers' society. But, if the merchants' chosen workers "were troubled or menaced, judicial authority, aided if necessary by military authority, would not fail to arrest and punish those who made themselves guilty of assaults or threats."[38] Julliany was almost certainly right that the state, if called in, would have backed the merchants and broken the dockworkers' monopoly by force: the restrictive practices of the dockworkers' society were a blatant violation of the criminal code and the proscription of "coalitions" in the le Chapelier law. Moreover, dock work did not require a long period of training, and there were always plenty of badly paid and underemployed men in the city—particularly, Italian immigrants—who could have been recruited to load ships at rates far below those maintained by the dockworkers' society. Yet the merchants ignored Julliany's suggestions and continued uncomplainingly to accept the society's restrictive regulations and pay its high rates. Given the reputation of Marseille's merchants as astute businessmen, it is puzzling that they continued to submit to this costly monopoly, especially after 1830, when the mayor who had been the dockworkers' patron had resigned, and the officials of the July Monarchy were loudly declaiming the virtues of laissez faire.

There are reasons to think that the merchants knew what they were doing, but explaining the merchants' toleration of the dockworkers' society requires a closer look at the organization of work on the docks. The loading of ships was carried out by teams of dockworkers, which might consist of ten to twenty men, headed by a master dockworker. (See Cover Illustration.) The masters were named not by the dockworkers' society but by the merchant whose goods were being handled. A master was the merchant's representative on the docks. He recruited, organized, supervised, and paid his team of workers, and often handled such formalities as customs clearance as well. According to tradition, each dockworker, master included, was to get an equal share of pay for the work accomplished. A master, however, could work for more than one merchant and could have more than one team working at a given time; he also probably benefited from various bonuses and gratuities from the merchant.[39] A few master dockworkers actually became well-to-do; twenty of them (out of perhaps one hundred) met the substantial property qualifications required to become electors of the July Monarchy in 1844.[40] But the masters and simple workmen were equal in the eyes of the dockworkers' society: no privileges attached to the position, and dues, fees,

[38] Julliany, *Essai sur le Commerce*, 3: 455.
[39] Nguyen, "Crise et vie," 38.
[40] Nguyen, "Crise et vie," 40.

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obligations, and benefits of membership were the same for all. Masters were overrepresented among officers of the society, but they never monopolized offices and there seem to have been very few complaints of prejudicial treatment by the ordinary workers. For whatever reasons—and the sources unfortunately are virtually silent on relations between masters and workers—teams of dockworkers seem generally to have worked together in harmony.

The master and his team of dockworkers were employed directly by the merchant whose goods were being handled. Marseille's merchants in the mid-nineteenth century carried on business much as they had in the eighteenth or seventeenth centuries. It was typical for each voyage of a ship to be financed by a different group of businessmen. The ship's captain, merchants whose goods were to be carried, and perhaps a banker or independent capitalist would become partners for a single voyage. When the ship returned, the profits or losses would be divided and the partnership dissolved, and a new partnership, normally including a different set of partners, would be established. A given merchant would be involved in several different ventures at once. He might have three or four ships coming in during a two-week period and then have none the next month.[41] A merchant's demand for labor was therefore extremely sporadic; he wanted to be able to hire a team of men when he needed them, and he wanted to be sure that they would do the work efficiently and honestly with a minimum of supervision. Rather than having to hire a full-time assistant to supervise operations, merchants could count on the master dockworkers and their teams to take charge of the work when it needed doing. The dockworkers' society itself guaranteed responsible and orderly work on the docks, mediating whatever disputes might arise, providing extra workers from the Muse when they were required, and generally overseeing loading and unloading of ships. From this perspective, the high wages paid to the dockworkers begin to seem a much better bargain. When a merchant hired a team of dockworkers, he was getting much more than willing muscles, he was also putting out or subcontracting the management of dock work to the masters and the dockworkers' society.

The high wages paid to dockworkers thus bought both physical labor and management services. But they also bought something else: security of the cargoes. The dockworkers' society ensured the honesty of the workers and effectively policed the docks. Pilferage is a chronic concern on the docks in all times and places, and the physical configuration of nineteenth-century Marseille posed special problems for its prevention. Marseille's port basin was a rectangular inlet nestled between steep hills on the north and south. The old city of Marseille was built on the slope of the hill immediately to the north of the port, and, by the end of the seventeenth century, the city surrounded the port on all sides. The old city neighborhoods to the north of the port, which were crowded, labyrinthine, poor,

[41] See Charles Carrière, *Négociants marseillais au XVIII^e siècle: Contribution à l'étude des économies maritimes*, 2 vols. (Marseille, 1973), 2: 875–984, for a lucid and detailed description of Marseille's mercantile capitalism in the eighteenth century.

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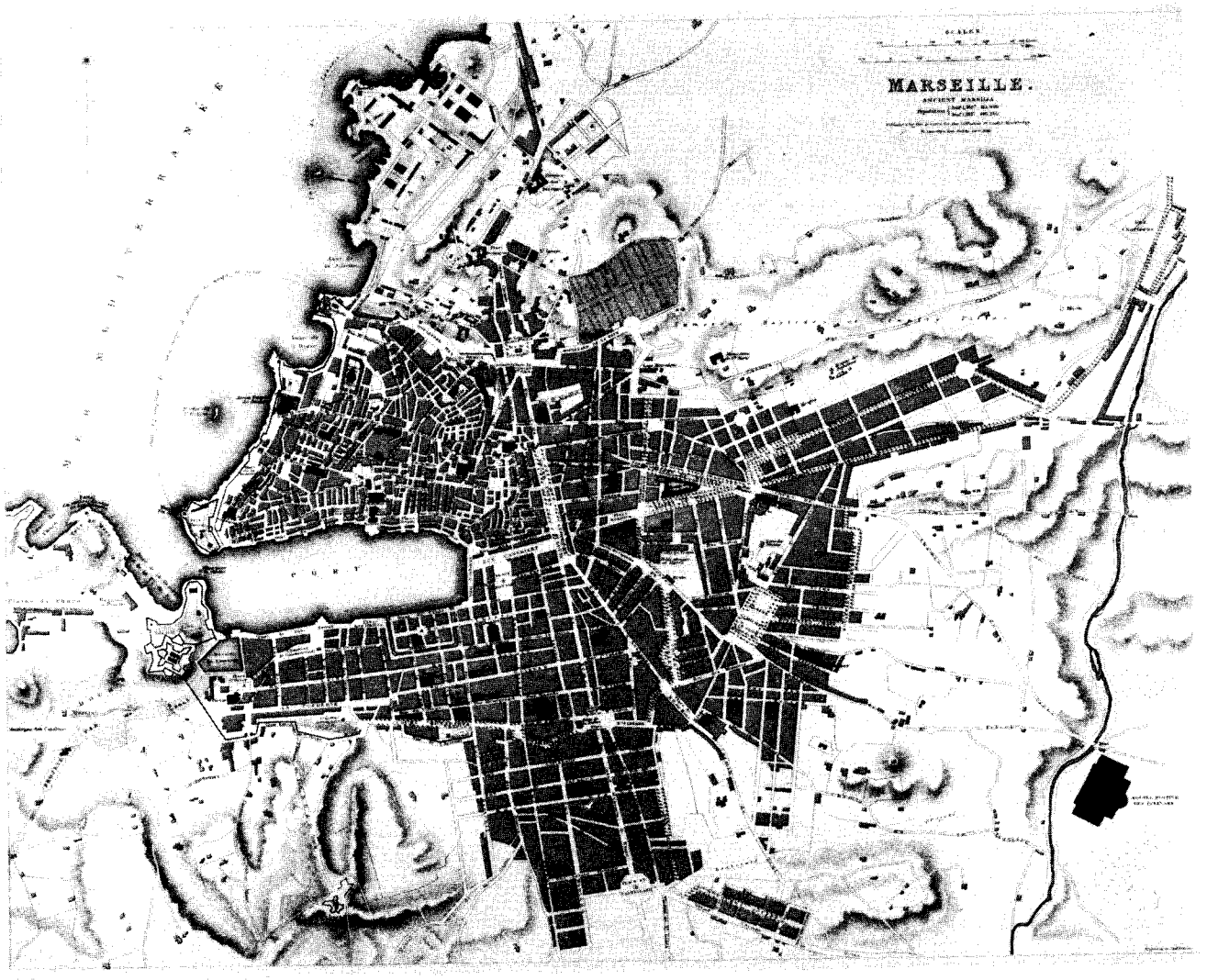


FIGURE 2: Marseille in 1837. Published by the Society for the Diffusion of Useful Knowledge.

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and dangerous, reached right down to the quays.[42] (See Figure 2.) The narrow quays, never very ample even in the eighteenth century, became extraordinarily encumbered with the explosive growth of maritime commerce in the nineteenth. In these circumstances, a dishonest dockworker could disappear into the old city in an instant with valuable cargoes. This danger was multiplied by the fact that goods being held for transshipment had to be carried to warehouses scattered all over the city. The dockworkers' honesty, consequently, was a major financial consideration for merchants.

The dockworkers' society ensured the honesty of its members not only by its general tone of discipline and order but by specific regulations. The statutes of the dockworkers' society stated that dockworkers convicted of theft by the courts were to be banished from the society and therefore from the docks as well. In addition, the society instituted its own proceedings against any dockworker denounced by the priors for having "permitted himself the baseness of embezzling, hiding, or holding back some portion of merchandise" or for "lacking trustworthiness in whatever way." If the case against the accused was judged by the society's general assembly to be proven, he was expelled from the society, and the outcome of the assembly's deliberation was sent to the Hôtel de Ville "so that no one will remain ignorant" of the expulsion. By this means, the society guarded itself and the docks against dishonest dockworkers even if the merchant whose goods had been filched declined to press charges in the courts.[43] The merchants were willing to pay the high wages enforced by the dockworkers' society in order to gain this protection against large losses from pilfering.

The dockworkers' society, then, was an integral part of the maritime economy of mid-nineteenth-century Marseille. The society had grown in tandem with Marseille's merchant community, and the relations they had established were mutually beneficial as long as no major changes took place in the overall organization of commerce. Even the huge rise in demand for dockworkers' labor that resulted from the commercial boom of the 1820s, 1830s, and 1840s failed to upset existing relations between dockworkers and merchants. Because the rising demand for labor was met by eliminating the dockworkers' irregularity of employment and by leaving other loading and carrying jobs to unskilled carters and *robeirols*, it resulted in no upward pressure on the dockworkers' piece-rate wages. Dockworkers' *earnings* rose substantially, yet the costs faced by a merchant hiring a team of dockworkers for a day in the 1840s or 1850s was virtually the same as it had been in the 1820s. For this reason, the dockworkers were able to gain an enormous advantage from the maritime boom without adversely affecting the profits of their employers, the merchants. Until the great transformation of the docks and the maritime economy that began to take shape toward the end of the 1850s, neither the merchants nor the municipality had any reason to challenge the

[42] On the geography of poverty and crime in Marseille, see Sewell, *Structure and Mobility*, 109–26, 228–32.

[43] The quotations are from the statutes of 1817. The 1853 statutes use somewhat different language and specify the procedures to be used in trying accused dockworkers in more detail, but essentially they follow the 1817 provisions, which in turn follow those of 1789. ADBdR: XIV M 25/1; AVM: 1 I 1/35.

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power of the dockworkers' society. The society relieved the merchants of responsibility for supervising loading and unloading operations, it protected them against the danger of rampant waterfront theft, and its members' growing prosperity in the 1830s and 1840s did not increase the merchants' costs. Consequently, the dockworkers were secure and prosperous, for the time being, in a protected niche.

IF THE EXPANSION OF COMMERCE POSED NO IMMEDIATE CHALLENGE to the customary organization of dock work in Marseille, it soon began to strain the physical capacity of the old port. The port was already overcrowded by the 1820s, and by the middle 1830s the situation became critical. Several stop-gap measures were undertaken, such as the razing of buildings along the northern edge of the port to expand the surface of the docks and dredging the port's southeastern corner to make room for another twenty to thirty ships. But, even after improvements, the docks measured only 3,200 meters for an annual traffic of 7,000–8,000 ships in the late 1840s. By contrast, Le Havre, Marseille's chief French competitor, had 5,920 meters of docks for fewer than 5,000 ships.[44] Minor improvements were insufficient to alleviate the problem; the traffic of the port grew far faster than the available space. Crowding was further exacerbated in the 1830s by the appearance of steamships, which were much larger than sailing vessels and difficult to accommodate in the confines of the old port.

It was clear that a new basin comparable in size to the old port was needed. Given the geography of Marseille, this meant a gigantic program of construction. The old port is the only part of the coastline of Marseille with natural shelter against storms, good access to the shore, and sufficient depth of water, all of which are required for a good port. The immediately adjacent coastline both to the north and to the southwest of the old port was so jagged, steep, and rocky as to make the difficulty and cost of construction formidable (see Figure 2). For eight years, from 1835 to 1843, engineers, entrepreneurs, the Chamber of Commerce, and representatives of the local and national government haggled over dozens of rival plans for new port basins, finally agreeing on the area immediately to the north of the old port. Work on the project began in 1844, and the new basin was already usable in 1847, when it was crowded by vessels carrying wheat to provision France's exhausted granaries. The new port was fully completed in 1853. As early as 1848, however, the local director of the government's civil engineering corps (*Ponts et chaussées*) had decided that a further northward extension was necessary.[45] The Revolution of 1848, and the financial insolvency and reduction in port traffic that it brought in its wake, interrupted plans for extension. By 1852, maritime commerce began to grow at its pre-1848 pace once again, and the expansion continued through the 1850s and 1860s (see Figure 1). By 1856, work began on

[44] Paul Masson, gen. ed., *Les Bouches-du-Rhône: Encyclopédie départementale*, vol. 9, *Le Mouvement économique* (Paris, 1922), 429–33, 437.
[45] Masson, *Les Bouches-du-Rhône*, 9: 446–47.

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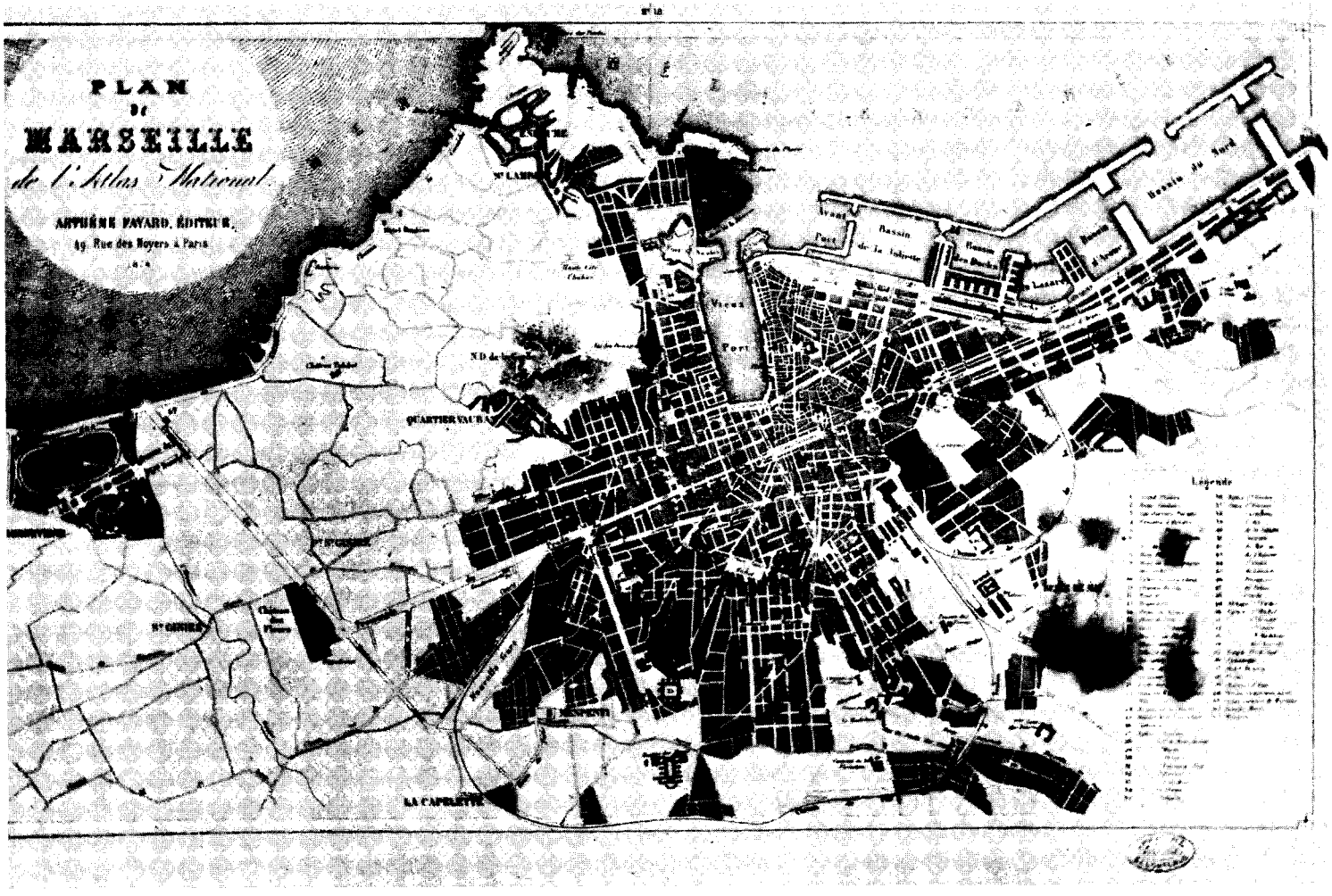


FIGURE 3: Marseille in 1874. Courtesy of the Archives de la Chambre de Commerce et d'Industrie de Marseille.

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a series of three new port basins stretching northward along the coast. (The old and new port basins can be seen clearly in Figure 3, a plan of Marseille in 1877.)

The basin completed in 1853 had no significant effect on the organization of dock work. The system that had been used in the old port was simply transferred to the new. But the basins begun in 1856 were a very different matter. The concession for building, equipping, and running these basins was awarded to a joint-stock company called the Compagnie des Docks et Entrepôts de Marseille. This company, which was financed by Parisian capital, revolutionized methods of handling cargoes.[46] First, the new dock was what was called a *dock à l'anglais,* that is, it introduced labor-saving equipment, of which steam-driven hydraulic cranes and lifts were the most important. Second, the quays were physically set off from the working-class neighborhoods of the old city. Third, all goods in transshipment were to be concentrated in warehouses in the new basins rather than being scattered through the city. Fourth, and most important of all, dockworkers were no longer to work in teams for the merchants whose goods were being unloaded: they had to become employees of the Compagnie des Docks. Work was to be organized and policed not by the masters and the dockworkers' society but by employees of the company.

The dockworkers quickly recognized that establishment of the new dock would gravely threaten their position. In 1858, they learned that all steamships would be required to use the as-yet-uncompleted docks as soon as provisional operations began. The dockworkers' society sent delegations to the company, the mayor, and the prefect to protest this regulation, which would effectively put the rapidly growing steamship trade beyond the society's reach. In January, 1859, the dockworkers' society even obtained an audience with Emperor Napoleon III, who answered their plea with vague, and in the end empty, promises of help: "Messieurs, you have done very well to count on me. I will do for your interests everything that may be in my power."[47] When the docks began provisional operations in the summer of 1859, a number of dockworkers went to work for the company. The society forbade its members to do so and expelled all who did. In other words, it threatened to withhold labor from the company, demanding that dockworkers be allowed to handle work for their merchants as before, without becoming the company's employees. The company, as yet in a weak economic position, negotiated a compromise that allowed society members to handle some kinds of work without becoming employees.[48] But both sides knew that this was only a truce in a long battle, and that the real showdown would come in 1864, when the new docks were scheduled to go into full operation.

During its long struggle with the company, the dockworkers' society did its best to cultivate public opinion. It obtained the signatures of 850 merchants on a

[46] Masson, *Les Bouches-du-Rhône,* 9: 450–52; Louis Girard, "La Politique des grands travaux à Marseille sous le Second Empire," in Chambre de Commerce de Marseille, *Marseille sous le Second Empire Exposition, conférences, colloque organisés à l'occasion du centenaire du Palais de la Bourse, 10–26 novembre 1960* (Paris, 1961); Nguyen, "Crise et vie," 79.

[47] ADBdR: XIV M 25/1.

[48] Nguyen, "Crise et vie," 81.

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petition addressed to the ministry of industry and commerce and won the support of the Chamber of Commerce, the bastion of Marseille's merchant community.[49] That it succeeded in gaining the support of the merchant community is impressive, since the Compagnie des Docks was able to offer considerably lower price-rates than the dockworkers' society. In part, this support is a measure of the longstanding ties between the merchants and the dockworkers, especially the master dockworkers. The dockworkers' society, in a memorandum addressed to Marseille's merchants in 1859, for example, made much of the paternal relations of "confidence" and "devotion" that existed between merchants and "their" dockworkers.[50] But the merchants had grievances of their own. In the 1850s and 1860s, Marseille's maritime commerce was completely restructured.[51] One important development was the rise of steam navigation and joint-stock steamship companies. Steamships, which accounted for only 10 percent of the cargoes entering the port of Marseille in 1840, accounted for 14 percent by 1850, 32 percent by 1860, and 44 percent by 1870.[52] The early steamships were often owned by individuals or simple partnerships, but these small operators were soon pushed aside by large joint-stock steamship companies. Between 1852 and 1865, four companies—the Messageries Impériales, the Société Générale de Transports Maritimes à Vapeur, the Compagnie Marseillaise de Navigation à Vapeur, and the Compagnie de Navigation Mixte—came to dominate steam shipping in Marseille, accounting for almost 90 percent of the city's steamer tonnage by 1869. Only one of these companies, the Compagnie Marseillaise, was controlled by local capital; the majority of the capital in the other three was from Paris or Lyon.[53] The rise of these steamship companies disrupted the traditional organization of commerce. Rather than forming a series of short-term partnerships for single voyages, merchants increasingly found themselves dealing with large bureaucratic shipping lines. Moreover, the steamship companies, together with the Compagnie des Docks, rivaled the power of the traditional merchant community and its organ, the Chamber of Commerce. Marseille's merchants, long the dominant force in the political and economic life of the city, were rapidly being displaced by "foreign" capital. "Marseille," as the scion of an old mercantile family remarked in 1863, "no longer seems to belong to herself."[54] Under these circumstances, the dockworkers' struggle against the Compagnie des Docks could be seen as a battle against foreign domination. As such, it won the support of the merchants and the Chamber of Commerce.

If the battle against the company strengthened the bonds between merchants and dockworkers, it gradually opened a rift between masters and ordinary dockworkers. It was the masters whose position was threatened most drastically by the new dock. Ordinary dockworkers stood to lose their collective self-governance

[49] Nguyen, "Crise et vie," 83.
[50] ADBdR: XIV M 25/1.
[51] Sewell, *Structure and Mobility*, 38–43.
[52] Masson, *Les Bouches-du-Rhône*, 9: 56, 74.
[53] Masson, *Les Bouches-du-Rhône*, 9: 67–76.
[54] Quoted in Pierre Guiral, "Quelque notes sur la politique des milieux d'affaires marseillais de 1815 à 1870," *Provence historique*, 7 (1957): 174.

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and to suffer a decline in earnings. But the master dockworkers risked the annihilation of their peculiar function and status. The masters were willing to make dramatic gestures to gain the support of the merchants. In 1859, they offered a 10 percent reduction in piece-rates that would come entirely from their own profits, without reducing the rates to be paid to the workers.[55] But, as early as 1859, some of the ordinary dockworkers regarded the victory of the company as inevitable and wished to negotiate with the company rather than refusing to work at the dock altogether. In this respect, it is interesting that the first thirty-seven dockworkers to sign up for work at the dock in August, 1859, had previously been employed not by merchants but by the Messageries Impériales steamship company; they had, in other words, already made their peace with the new bureaucratic capitalist order. Another 290 workers came to sign up with the dock shortly after; all of these were expelled from the dockworkers' society.[56] Throughout the struggle, it was always the masters who insisted on total non-cooperation, whereas at least some of the workers were willing to accept work at the dock if the terms were sufficiently favorable.

In the summer of 1864, the dock began full-scale operation. Once again, the Compagnie des Docks insisted that anyone engaged in loading and unloading of ships be an employee of the dock, and once again the dockworkers' society responded by refusing to work under the company's conditions. This time, the company was prepared; it simply staffed the docks with unskilled laborers, mostly Italian immigrants, and offered merchants lower rates than the dockworkers' society could match. Once again, a number of dockworkers took work with the company in defiance of the society and were duly expelled. They brought suit, charging that the society had no right to exclude them for exercising their liberty to work where they wished. The court, as Julliany had predicted some twenty years earlier, ruled on behalf of the expelled men, striking a mortal blow to the society's powers. Not long after this decision, the emperor passed through Marseille and was greeted by a demonstration of some 5,000 dockworkers and supporters. One of the society's priors addressed the emperor, reminding him of the promises he had made in 1859, and ended his speech with a cry of "Vive l'Empereur!" Louis Napoleon once again promised to look into the affair, and a few months later the same prior went to Paris to negotiate a solution. But the solution he brought back to Marseille proved to be unacceptable to the society, and the effort ended in mutual recrimination.[57]

The dockworkers' struggle ended in victory for the Compagnie des Docks. Most of the society's members eventually went to work for the company, and the society, stripped of its monopoly, faded into an ordinary mutual aid society that administered sickness and retirement benefits. The merchants, in spite of their preference for the dockworkers' society, soon made their peace with the company as well. The company, after all, was able to offer them virtually everything the

[55] ADBdR: XIV M 25/1.
[56] ADBdR: XIV M 25/1.
[57] Nguyen, "Les Portefaix marseillais," 363–69.

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society had offered in the past. It provided workers, organized and supervised dock work, maintained secure and efficient warehouses, and protected the merchants' goods against losses through theft—a task made easier by the physical separation of the new docks from the neighborhoods of the old city. Moreover, it could offer all this at a lower rate than the dockworkers' old price schedule. The merchants lost the personal satisfaction of paternal relations with trusted master dockworkers and had to accustom themselves to the bureaucracy of the new company. But, unlike that of the dockworkers, their own privileged and lucrative position in maritime trade was not genuinely damaged. A new level of capitalist organization had come to Marseille's docks; to the merchants, it meant a change in the tone and character of their work and the loss of their once unchallenged supremacy in the city's maritime economy. To the dockworkers, it meant much more: the destruction of the cherished privileges of their society and the reduction of its members to ordinary proletarian wage laborers.

The uneven capitalist development that had fortified the dockworkers' privileged niche in the first half of the nineteenth century destroyed it in the 1860s. The general development of French, European, and world capitalism caused a massive expansion in Marseille's maritime trade in the nineteenth century. For some decades, this advance in capitalist development did not cause a corresponding advance in the methods of dock work. On the contrary, it actually strengthened an archaic form of labor organization on the docks: the dockworkers' society, which was in fact a carryover from the guild system of the Old Regime. In this case, the unevenness that characterized all capitalist development also gave rise to combined development: the strengthening of archaic forms in symbiosis with an advanced sector. But, in the end, the exigencies of continuing capitalist development, that is, the need for more space in the port, shattered archaic forms and obliterated the temporarily privileged position of the dockworkers. Marseille's dockworkers were a particular case but a case that fits a very general pattern. The intrinsic and inescapable unevenness of capitalist development promiscuously creates privileged niches for laborers who are advantageously placed in a particular phase of development. But it also, no less promiscuously, destroys the same privileged niches it had created in earlier phases.

AT FIRST GLANCE, IT WOULD SEEM THAT THE ECONOMIC VICISSITUDES of work had a parallel effect on the dockworkers' politics. The role of dockworkers in the three great revolutionary crises of the nineteenth century (1830–1834, 1848–1851, and 1870–1871) appears to correspond admirably to the evolution of their economic circumstances. The dockworkers seem to have been radical, or at least politically restive, in the first crisis, before their rise to the high prosperity of mid-century; to have been notoriously reactionary at mid-century, when their privileges were at their height; and to have been ardent revolutionaries at the time of the Commune, when their society had been crushed, and they had been definitively reduced to proletarian status.

During the crisis that extended from the July Revolution of 1830 to the suppression of the Lyonnais and Parisian workers' rebellions of April, 1834,

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Marseille was not the center of a major radical movement. In fact, the local authorities of the new July Monarchy were at first much more concerned about the possibility of a popular uprising in favor of the ousted Bourbon monarchical regime than about republican agitation. By 1833, however, a chapter of the revolutionary Society of the Rights of Man had been founded, and the police responded by infiltrating it with an informer. In April of 1834, at the time of the Lyonnais and Parisian insurrections, Marseille's society deliberated about launching a revolt of its own. According to the informer's report, among the most ardent advocates of violent action were three dockworkers, who claimed to be able "to direct at their will at least 200 of their colleagues" in the event of a revolt. This claim may have been wildly exaggerated; we will never know, because the Society of the Rights of Man finally decided against an insurrection. But it suggests that a sizable minority of the dockworkers had been touched by the radical agitations of the period.[58]

The dockworkers' growing prosperity during the following decade and a half seems to have wiped out any remnants of radicalism. By February, 1848, when the July Monarchy was overthrown and replaced by a republic, any claim that dockworkers would join a radical insurrection would have been dismissed as ridiculous; all political factions assumed that dockworkers were unshakably conservative. This assumption explains the way they were treated by the mayor of the fallen regime during his final days in power. The news of the Parisian insurrection reached Marseille on 24 February, but it was not until 1 March that the commissioner sent from Paris by the provisional government could arrive and establish republican power in the city. During this interval, the mayor did what he could to strengthen the hand of conservatives. Most important, he called in supporters of the fallen regime, set them up as companies in the national guard, and distributed to them all the city's available rifles. Nearly all the companies created by this maneuver were composed of merchants, professional men, shopkeepers, and clerks, but the dockworkers were armed to a man and allowed to form a special company of their own. This was in the sharpest contrast to the treatment received by other workers. When they came to the city hall to join the national guard, they were told that no arms were available. As one republican militant wrote, "If the dockworkers were admitted, it was in the thought, which [the authorities] did not even trouble to dissimulate, that these men of the people, feudatories of their patrons the merchants, would serve to combat that other people, the workers, whose very name brought on the shivers."[59] In 1848, conservatives and revolutionaries agreed that the dockworkers could be relied on to act as a counterrevolutionary force.

But the dockworkers' conservativism did not withstand the disasters of the 1860s. Their reduction from a privileged elite to ordinary proletarians made them

[58] ADBdR: M6/334.

[59] Prosper Dubosc, *Quatre mois de République à Marseille, 24 fevrier–24 juin* (Marseille, 1848), 5. This highly irregular, last-minute formation of a conservative national guard was documented in a detailed report by the revolutionary municipal commission's committee on the national guard. AVM: 1 D 73, session of 10 June 1848.

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notoriously hostile to the Imperial regime that had succeeded the Second Republic, all the more so because the emperor himself had twice deceived them during their struggle with the Compagnie des Docks by giving vague but empty promises of help. It is therefore hardly surprising that dockworkers figured prominently in a republican demonstration in Marseille during July of 1870, two months before the overthrow of the empire and the establishment of the Third Republic, or that several dockworkers were active in Marseille's Commune in 1871. One of these, Etienne père, was the Commune's second in command and was condemned to death after the suppression of the Commune, although his sentence was subsequently commuted.[60] The correlation with politics therefore appears straightforward: if the rise of the dockworkers to prosperity made them conservative by 1848, their fall from prosperity led them to political radicalism at the time of the Commune.

On closer observation, this intimate association between economic situation and political behavior turns out to be seriously flawed. It is true that the dockworkers were staunch conservatives in February of 1848 and exemplary revolutionaries in 1871. But it appears that their shift to the left began during the period of the Second Republic, several years before the demise of their economic fortunes. The evidence for this shift is scattered, incomplete, and not entirely conclusive, but it all seems to point toward a general drift to the left, beginning hesitantly in the spring of 1848, accelerating in 1849, and sustained up to and beyond Louis Napoleon's coup d'etat in 1851. The first sign of such a drift occurred in June, 1848, when radical workers in Marseille revolted at the time of the far more famous Parisian June Days. The dockworkers' national guard company—originally formed as a bastion of reaction—refused to respond to orders on discovering that they might be asked to fire on fellow workers.[61] A year later, there was a sign that the dockworkers were moving toward a more insurgent position. In June of 1849, some 500 dockworkers held a banquet in honor of Louis Astouin, a dockworker who had written a widely acclaimed book of poetry and who had just been defeated in a bid for reelection to the National Assembly on the democratic ticket. The banquet ended with the entire crowd shouting "Vive la République démocratique et sociale!" and some 114 francs were collected for the families of the insurrectionaries of the prior June who remained in custody.[62] Neither of these actions would have been thinkable in the early days of the Second Republic.

Sparse but intriguing quantitative figures on dockworkers' participation in revolutionary activities in the decade following the Revolution of 1848 seem to indicate that the drift to the left that began in 1848 and 1849 was not slowed by

[60] Nguyen, "Les Portefaix marseillais," 376–80. Antoine Olivesi, in his history of Marseille's Commune, describes Etienne as "a pure revolutionary with the soul of an apostle," *La Commune de 1871 à Marseille et ses origines* (Paris, 1950), 76.

[61] Dubosc, *Quatre mois de République*, 48.

[62] There is an account of the banquet in the republican newspaper *La Voix du peuple*, 8 June 1849. According to my quantitative analysis of the 1851 census, there were about 1,500 dockworkers in Marseille in 1848. The 500 present at this republican banquet therefore represented about one-third of all dockworkers in the city.

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TABLE 2
Participation of Dockworkers in Radical Politics

	1 *dockworker participants/all participants*	**2** *dockworkers as percent of participants*	**3** *dockworkers as percent of work force*	**4** *dockworker participation* **2/3**
Insurrection, 1848	3/270	1.1	2.9	0.4
Coup d'etat, 1851	6/297	2.0	2.9	0.7
"Dangerous individuals,"				
1853–59	18/346	5.2	2.9	1.8
Miscellaneous political				
offenses,				
1848–49	0/20	0.0	2.9	0.0
1850–51	2/51	3.9	2.9	1.3
1852–59	8/85	9.4	2.9	3.2

the mounting repression of the final years of the Second Republic or even by the establishment of Louis Napoleon's embryonic police state (see Table 2).

Only three of the 270 men who were convicted of participating in Marseille's June 1848 insurrection were dockworkers. Dockworkers thus made up 1.1 percent of the insurrectionaries, as against 2.9 percent of the adult male population. Dockworkers may have been reluctant to fire on other workers, but few were ready to join in an armed insurrectionary movement. Among the 297 radicals rounded up after Louis Napolean's coup d'etat in 1851 were six dockworkers. This was 2 percent, still below the dockworkers' percentage of the population but double the 1848 percentage. When the police drew up lists in 1853, 1855, and 1858, composed of "dangerous individuals" who were to be rounded up if an insurrection should take place, dockworkers made up eighteen of 346 names, which is 5.2 percent, or well above their proportion in the population. The same development, but even more pronounced, can be seen in the identities of men convicted of assorted political offenses over the same years. The offenses included were quite diverse, the most common being politically motivated acts of violence against police or other authorities, membership in secret revolutionary societies, or singing subversive songs or uttering "seditious cries" in the streets. No dockworker was among the twenty convicted of such offenses in 1848 and 1849. In 1850 and 1851, there were two dockworkers among the fifty-one men convicted, a number somewhat above their proportion in the population. In the first eight years of the Second Empire, dockworkers made up eight of the eighty-five men convicted of political offenses, about triple their proportion in the population. These figures are obviously insufficient as measures of the overall political opinions and behavior of the dockworkers, but they would seem to confirm that dockworkers were drifting to the left in the years following 1848.

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There is one final indication—once again, hardly conclusive in itself—of the dockworkers' shift to the left between 1848 and the early 1850s. This is a remark made by Armand Audiganne, a prominent Parisian social investigator, in 1855, in his monumental *Les Populations ouvrières et les industries de la France.* He reported that the revolutionary movement of 1848 in Marseille "received support above all from the corporation of the dockworkers, justly famous for their riotous temperament."[63] This manifestly false statement would seem to be explained by the fact that he visited Marseille in 1852 or 1853 and projected the then-evident radicalism of the dockworkers backward in time to an era when, in fact, they were regarded by democrats and reactionaries alike as staunch supporters of the existing social and political order.

All the evidence points to a significant radicalization of dockworkers' political opinions and behavior between 1848 and the mid-1850s, a decade before the drastic restructuring of work reduced the dockworkers to proletarians. It therefore appears that changes in dockworker politics cannot be explained as a simple reflex of proletarianization. Instead, the seemingly paradoxical rise of dockworker radicalism over the decade following the Revolution of 1848 requires a complex explanation, one that recognizes the simultaneous autonomy and interrelationship of economic, political, and social factors.

Let us begin with economics. Although the dockworkers' favorable structural position in the maritime economy was not challenged in the late 1840s and 1850s, their economic well-being was sharply affected by short-term fluctuations. The traffic of Marseille's port fell precipitously in 1848 and remained low for several years (see Figure 1). The revolutionary upheavals of 1848 in France and Europe caused a general panic among the possessing classes; credit dried up and investment fell, causing a depression in industry and trade. It was not until 1852, after Louis Napoleon's coup d'etat had ended the fear of political instability, that the traffic of the port rebounded to the level of the middle 1840s.[64] The long commercial depression surely increased unemployment and sharply cut incomes on the docks, reducing the dockworkers to economic circumstances more or less comparable to those of the early 1830s, when, as we have seen, they were also involved in revolutionary agitation. The dockworkers were not thrown into desperate poverty by the depression, and the Muse operated to distribute available employment equitably. Indeed, the Muse probably made the impact of the depression less severe for the dockworkers than for most of the city's workers. Nevertheless, lower incomes and high levels of unemployment must have made dockworkers far more receptive to the republican and socialist agitation of the era than would have been the case had employment remained high. This economically induced receptivity undoubtedly helps to explain the dockworkers' initial drift to the left from 1848 to 1851.

[63] This quotation appears on p. 154 of vol. 2 of the 2d edn. of Audiganne's work, which was published in 1860. It also appeared in the 1st edn., dated 1855.

[64] Pierre Guiral, "Le Cas d'un grand port de commerce: Marseille," in *Aspects de la crise et de la dépression de l'économie française au milieu de XIX^e siècle, 1846–1851*, ed. C.-E. Labrousse, vol. 19 of *Bibliothèque de la révolution de 1848* (La Roche-sur-Yon, 1956), 200–25.

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These economic changes were paralleled by (and in part caused by) autonomous shifts in the French political system. Most recent literature on the autonomy, or relative autonomy, of politics has focused on the autonomy of the state. States, this literature argues, have their own interests, structures, and developmental tendencies that cannot be reduced to the interests of the dominant class, classes, or groups. Explaining historical change therefore requires attention not only to the changing conditions of the classes and class fractions that make up a society but also to the changing form and functioning of states.[65] The importance of changes in political structures is clear in revolutionary upheavals. The Revolution of 1848 drastically altered the structure of the French state. The sudden victory of the February 1848 insurrection overthrew a monarch, dissolved a legislative body, and threw existing state institutions into disarray. It created a new but insecure provisional government, which instituted universal male suffrage and abolished censorship and restrictions on the formation of political associations and trade unions. Under pressure from Parisian workers, it also declared a new fundamental right, the right to work, and established the Luxembourg Commission, headed by socialist Louis Blanc, which was to prepare a new "organization of labor" for the country. The new and rather chaotic structure of the French state in the immediate aftermath of the February Revolution enormously expanded the possibilities for political action by workers. Granted the vote, allowed to organize in political clubs and trade associations, and promised a fundamental reorganization of the economy, workers in cities throughout France responded with a frenetic burst of political activity. A fundamental cause of the radicalization of French workers in general, this change in the structure of the state must have affected Marseille's dockworkers.

The February Revolution also brought about another closely related kind of change in the nature of politics that is seldom considered by those who argue for the autonomy of the state: a transformation of political discourse.[66] If the hard economic times of the Second Republic made dockworkers more receptive to messages critical of the status quo, revolutionary politics changed the character and increased the volume of such messages.[67] The Revolution of 1848 made "worker" (*ouvrier* or *travailleur*) a politically potent term and made the fact that one belonged to the socioeconomic category of manual workers politically relevant in a way it had not been before. In Marseille, as elsewhere in France in the spring of 1848, not only socialists but public officials, moderates, and even the most reactionary monarchists addressed a blizzard of pamphlets, newspaper articles, speeches, handbills, and proclamations "to the workers." In Louis Althusser's terminology,

[65] See, for example, Peter B. Evans, Dietrich Rueschemeyer, and Theda Skocpol, *Bringing the State Back In* (Cambridge, 1985); and Theda Skocpol, *States and Social Revolutions: A Comparative Analysis of France, Russia, and China* (Cambridge, 1979).

[66] An exchange on these issues concerning the French Revolution is in William H. Sewell, Jr., "Ideologies and Social Revolutions: Reflections on the French Case," *Journal of Modern History*, 57 (March 1985): 57–85; and Theda Skocpol, "Cultural Idioms and Political Ideologies in the Revolutionary Reconstructions of State Power: A Rejoinder to Sewell," *Journal of Modern History*, 57 (March 1985): 86–96.

[67] I have argued this at some length in *Work and Revolution*, chap. 11.

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one could say that after February, 1848, the utterance "worker" insistently interpollated or hailed workers as ideologically defined political subjects.[68] Bombarded by this outpouring of ideological discourse, even the dockworkers, who had been monarchist or apolitical before 1848, surely were aware that the revolution was promising to raise manual labor to a new dignity in the state and must have pondered what their unquestioned status as workers implied for their own role in politics.

Much of the discourse directed specifically at Marseille's dockworkers in the spring of 1848 came from conservative quarters and praised them for their "moderation." For example, on 26 February, the day the dockworkers were incorporated into the national guard, the conservative newspaper *Le Nouvelliste* remarked that the dockworkers' firm repudiation of the "tumultuous demonstrations that have alarmed this city" had

> honored in the highest degree the working population [*population ouvrière*] of our city . . . This evening more than 2,000 of these estimable workers [*ouvriers*], armed and incorporated into the national guard, will efficaciously stand guard to assure due respect to persons and property . . . We do not doubt that the noble example of the dockworkers' corporation will be followed by the entire working population [*population ouvrière*] of Marseille, because nowhere else, and we say this with a just pride, do the laboring classes [*classes laborieuses*] offer, to the same extent as ours, such guaranties of morality and devotion to the sacred bonds of the family, from which flow essentially all instincts of order and legality.[69]

This editorial not only attempts to reinforce the dockworkers' conservativism but holds up the dockworkers as a model for all of Marseille's workers, who, interestingly, are praised not so much for their political conservatism as for their apolitical attachment to family bonds, which is assumed to be a guarantee of good political behavior. Yet even this text paradoxically contributes to the dockworkers' identity as workers and, in spite of its reactionary intent, makes this identity politically potent.

From the very beginning of the revolution, Marseille's conservatives, by ostentatiously adopting the dockworkers as the apotheosis of the apolitical *sage ouvrier*, unwittingly contributed to their politicization. Once dockworkers had been encouraged to think of themselves as somehow exemplary of workers in general, they could not be prevented from recognizing that republicans and socialists were hailing all workers as constituents of a new and better state and society, a "democratic and social republic" in which labor would be properly "organized" and duly rewarded as the basis of all wealth. By June of 1848, the republicans and socialists had not succeeded in constituting dockworkers as revolutionaries. But the fact that the dockworkers' unit of the national guard refused to report for duty at the time of the June insurrection because they were unwilling to fire on "fellow workers" indicates that they had begun to accept "worker" as a political identity and to act on that identity even against an express command of the forces of "order and legality."

[68] Louis Althusser, "Ideology and Ideological State Apparatuses (Notes towards an Investigation)," in *Lenin and Philosophy and Other Essays* (London, 1971).
[69] *Le Nouvelliste*, 26 February 1848.

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The shift in state structure and political discourse that took place in the spring of 1848, together with the sharp rise of unemployment, appears to have moved the dockworkers beyond the reactionary docility attributed to them by both monarchists and republicans in February. But it was not until 1849 or later that dockworkers began to appear with some frequency in the ranks of Marseille's militant republicans and socialists. This delayed conversion of dockworkers to the radical movement was in fact unique in Marseille. The great era of political radicalization for workers was the turbulent spring of 1848, not the increasingly repressive years that followed. The figures on participation in Marseille's revolutionary movement show that, with the exception of the dockworkers, trades that were radical in 1851 or the middle 1850s had already been radical at the time of the June 1848 rebellion. The dockworkers were the only significant working-class trade that had been conservative in June of 1848 but turned radical later. Hence the factors that explain the general radicalization of workers in 1848 cannot fully account for the particular case of the dockworkers.

Explaining the changing politics of dockworkers requires that we recognize the autonomy of politics in another sense: that in addition to considering changes in state structures and political discourses, we must also consider the unique details of their political history. From 1849 to 1851, the political history of the dockworkers was closely tied to the surprising career of Louis Astouin, the dockworker poet-politician. Astouin was initially named to the monarchist slate of candidates in the legislative elections of April, 1848. The monarchists chose him because of his prominence and popularity as a worker-poet, assuming that his apparent lack of pronounced political views indicated the docile conservatism for which his fellow dockworkers were famous. But in the National Assembly he turned out to be a sincere, if moderate, republican. He was therefore dropped from the monarchist list for the elections of 1849 and replaced by another dockworker of known reactionary opinions. The democrats, however, named Astouin to their list, and he responded by moving considerably to the left and espousing the cause of social democracy. Astouin became a tireless campaigner. Although narrowly defeated in the 1849 election, he continued to work for the democratic and social republic, until Louis Napoleon's coup d'etat in December of 1851. Astouin's towering prestige among the dockworkers, together with his diligent and incessant proselytising, was surely a major source of the new political insurgency that characterized the dockworkers after 1849.[70]

Three major factors, then, must be taken into account to explain the radicalization of Marseille's dockworkers in the years following 1848. First, the change was made possible by a revolutionary transformation of the state and a consequent upsurge in radical discourse that established "the worker" as a constituent of a new "democratic and social republic." This new vision of work and politics so dominated the discourse of the spring of 1848 that even conservative workers such as the dockworkers could not escape a new politicized working-class

[70] Astouin's career can be traced from the spring of 1849 to the fall of 1851 by occasional items in the police files and by articles in *La Voix du peuple* and its successor, *Le Peuple*.

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identity. Second, the extended maritime depression of the Second Republic doubtless made dockworkers more receptive to the radical message than they would have been in a period of full employment. Finally, the particular political career of Louis Astouin meant that the gospel of the "democratic and social republic" reached the dockworkers with special authority and intensity between 1849 and 1851. The confluence of these general and particular conditions had the effect of transforming dockworkers from notorious reactionaries to active republicans by the end of the Second Republic.

None of these factors, however, can explain the increasing prominence of dockworker militants in the underground republican movement that followed Louis Napoleon's coup d'etat and the establishment of the Second Empire. The maritime economy, after four years of depression, turned sharply upward in 1852 and remained relatively buoyant to the end of the decade. After 1851, republican and socialist discourse was choked off by the tight censorship and oppressive police of the Imperial state. And Louis Astouin was arrested after the coup d'etat in 1851, placed in internment in Valence and then in Besançon, and kept under close police surveillance when he returned to Marseille in 1853; he was no longer able to participate actively in the now illegal republican movement.[71] Yet the dockworkers did not relapse into their deferential conservatism of the 1840s. To judge from evidence in the archives of repression, summarized in Table 2, dockworkers were among the most active trades in the underground republican movement that took shape in Marseille in the 1850s; their participation in radical politics became more prominent as repression got more severe. The most likely explanation for this increasing prominence is the extraordinary solidity of the dockworkers' social organization. The dockworkers' unparalleled organizational experience and the density of their personal and institutional contacts made them unusually capable of sustaining the organizational efforts required in the political underground. The dockworkers' initial conversion to radical politics may have been slow, but, once they were committed to the democratic and social republic, their superior command of social resources moved them to the center of the political struggle.

The dockworkers' politics was hardly a reflex of their relation to the means of production. Socioeconomic conditions were far from irrelevant, but the evolution of dockworker politics cannot be explained without reference both to transformations in state structure and political discourse and to the details of politics and personalities in Marseille. To explain how Marseille's dockworkers were radicalized requires not just an examination of their changing economic circumstances but the construction of a multi-causal narrative that takes into account, weighs, and attempts to establish the joint and several explanatory powers of social, economic, and political factors.

[71] Astouin died in 1855 at the age of thirty-three. His death was attributed to an illness he contracted in 1853, not long after his return to Marseille, when he threw himself into the cold waters of the harbor to save a drowning child. Masson, *Les Bouches-du-Rhône*, vol. 11, *Biographies* (Marseille, 1913), 27.

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Does this mean that declaring the autonomy of politics is just another form of the call for a "revival of narrative"?[72] To some degree, perhaps it does: any attempt to explain historical change in a non-reductionist fashion implies a "narrative" concern for sequence, contingency, and agency. But the autonomy of politics should not be used as a license for historians to revel atheoretically in the particularity of each case's sequence of events or to resurrect (as it might seem I have done with Louis Astouin) the "great man" theory of history. The account of dockworker radicalization that I have sketched out here is not a return to old-fashioned narrative history but rather a theoretically motivated response to the (as yet insufficiently recognized) crisis of labor history's reigning explanatory strategy.

If capitalist development is uneven in the sense I have been arguing in this essay—that is, if it produces not an increasingly solid and uniform proletarian continent but a continually changing archipelago of variegated working-class categories—then the appropriate explanatory strategy for labor historians is not to look for evidence of proletarianization behind every surge of working-class political radicalism but to ask how and why workers with widely varying economic trajectories and work-place experiences could successfully be constituted as political insurgents. For some trades, in some historical instances, for example, Christopher Johnson's French tailors in the 1830s and 1840s, proletarianization surely is the most important single answer. But we would be wrong to think that the tailors epitomized nineteenth-century working-class experience. This study of Marseille's dockworkers indicates that even highly privileged workers could, under certain conditions, be induced to identify themselves with less fortunate workers and struggle for a radical transformation of the social order. If historians are to understand those occasions when a wide variety of workers joined radical revolts, such as the French workers' insurgencies of 1833–1834, 1848–1851, and 1870–1871, the English Chartist movement, the New York labor uprising of 1850, or the Russian revolutions of 1905 and 1917, we must ask how economic changes, transformations of state structures and political discourse, new or preexisting networks of social relations, and purposive actions by prominent or strategically placed persons or groups made possible the construction—at least for a time—of a common working-class political identity and program. Such a strategy should also help us explain why, in a capitalist economy that supposedly produces an increasingly uniform proletariat, these triumphs of class unity have repeatedly proved ephemeral. The case of Marseille's dockworkers should help us to see that socialism and class consciousness are contingent and fragile achievements of political struggle, not necessary and automatic products of the development of capitalism.

[72] Lawrence Stone, "The Revival of Narrative: Reflections on a New Old History," *Past and Present*, 85 (November 1979): 3–24; E. J. Hobsbawm, "The Revival of Narrative: Some Comments," *Past and Present*, 86 (February 1980): 3–8; and Philip Abrams, "History, Sociology, Historical Sociology," *Past and Present*, 87 (May 1980): 3–16.

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The Crowd in the French Revolution of February, 1848

MARK TRAUGOTT

In February of 1848, the people of Paris rose in revolt against the constitutional monarchy of Louis Philippe. Although in retrospect it was easy to appreciate how the ground had been prepared by the calamitous economic crisis that began in 1845–1846 and by the political reform movement launched late in 1847, the February Revolution appeared to stun even those best apprised of the French situation. Tocqueville himself, whose reputation for political prescience had been enhanced by a 29 January speech before the Chamber of Deputies warning of a "revolutionary wind in the air," later conceded that he spoke more to rouse his fellow deputies from their complacency than out of any concrete sense of what was soon to follow.

According to most contemporary accounts, the February insurgents were drawn from a broad spectrum of the Parisian population and, with the exception of the municipal guard's determined efforts, encountered little serious resistance.[1] Of course, assertions of the one-sided character of such conflicts frequently prove to be part of the creative reconstruction of events that follows a successful challenge to the power of the state. Still, as one indicator of the breadth of support for this insurrection, analysts could point to the fact that, between the first overt hostilities and the king's ignominious departure from the capital, barely forty-eight hours had elapsed. It is, in part, this contrast between the apparent unanimity of the population's response and the failure of contemporary observers to anticipate the insurrection's outbreak that accounts for the fascination of the February Days to students of revolutionary movements.

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[1] For a comprehensive and balanced contemporary account of the February Revolution, see Daniel Stern, *Histoire de la Révolution de 1848* (Paris, 1862). Alexis de Tocqueville's *Recollections* (1893; rpt. edn., Garden City, N.Y., 1971) and Karl Marx's "The Class Struggles in France, 1848–1852," although of special interest for the political or analytical perspective they bring to their subject, are less satisfactory as purely descriptive accounts. The best secondary analysis of the February Days proper is still Albert Crémieux, *La Révolution de février: Etude critique sur les journées des 21, 22, 23, et 24 février* (Paris, 1912). Also valuable are the accounts written by key participants in the events themselves, most notably Louis Blanc, *Pages d'histoire de la révolution de février 1848* (Paris, 1850); Alphonse de Lamartine, *Histoire de la révolution de 1848* (Paris, 1849); and Marc Caussidière, *Mémoires de Caussidière, ex-préfet de police et représentant du peuple* (Paris, 1849). Readers who would like a brief, English-language summary of the principal events might turn to Georges Duveau, *1848: The Making of a Revolution* (New York, 1967) or Priscilla Robertson, *Revolutions of 1848: A Social History* (Princeton, N.J., 1967).

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Another element of originality in the Revolution of 1848 derives from the February insurrection having toppled a regime that had itself been created by popular revolt. The July Days of 1830 had ended forever the rule of the Bourbons and raised hopes among republicans that a far more democratic form of government would be instituted. Although these hopes were disappointed by the compromise leading to the accession of the Orleanist dynasty, Louis Philippe's eighteen-year reign was far more liberal than what had preceded it and long appeared to enjoy a considerable measure of popular support and political stability. But with the February Revolution a circle of popular insurrection was closed, and the republican faction—which, though small, was said to be heavily represented among the barricade fighters—resolved that the cycle would not begin anew with elite manipulations of the sort that had thwarted the people's will in 1830.

Despite the existence of excellent narrative accounts, the February Days have been largely ignored by social historians of the past two decades. For each of the three other major insurrections of nineteenth-century Paris—July of 1830, June of 1848, and May of 1871—there exists at least a summary sketch of participants' backgrounds and an analysis, more or less rigorous, of the reasons for the occurrence of the uprisings at specific junctures in the nineteenth century. Only in the case of the February Revolution do we lack a systematic description of participants that might contextualize it in the light of what social history has taught us about the process of revolutionary mobilization.

Two reasons for this relative neglect seem obvious. First, the insurrection of February has been overshadowed by that of June. The February Revolution overthrew a regime, to be sure, but was too one-sided and short-lived to generate a comparable sense of historical drama. Its successor, on the other hand, appeared to pit key class formations in a life-or-death struggle and was widely seen by contemporary observers as diverse as Marx and Tocqueville as marking a historical departure. Through their interpretations, which exert a continuing influence on our understanding of the revolutionary process, the impact of the events of June has only been magnified, while, as an unintended consequence, the significance of the February insurrection has been diminished. Second, like other "successful" insurrections, the events of February failed to generate the most desirable kinds of historical records. Although the June insurrection of 1848 and the Paris Commune of 1871 would be counted watersheds of nineteenth-century French history by any standard, they also present the social historian with this signal advantage: these failed insurrections created a mass of invaluable documentation as a by-product of authorities' efforts to search out and punish the rebels.

Quite different is the outcome of successful insurrections like those of July, 1830 and February, 1848. In the period of exaltation that immediately follows, participants may trade stories of their exploits in the local bistro, but they typically resume their daily routines without ever setting down a record of their activities in any enduring form. Those who played salient roles in key events may become the object of highly embellished verbal accounts or, in rare cases, of celebratory articles in contemporary periodicals. It is true that the publicly acknowledged

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leaders of an uprising are likely to take a hand in the creation of new institutions, leading a rare few to commit their experiences to paper in the form of memoirs. Still, when judged against the judicial dossiers prepared in painstaking detail for everyone arrested following a failed insurrection, these meager written products are likely to be highly unsystematic, unreliable, or unrepresentative. As a consequence, it may prove difficult or impossible to establish a comprehensive and trustworthy picture of those who participated or to answer even the most basic questions one might pose concerning the social origins of insurgents.

Fortunately, in the case of February, 1848, there exists a partial solution to these difficulties. Following the February Days, the newly founded provisional government set up a Commission on Compensation. Its purpose was to review applications submitted by survivors of those killed or by those wounded or otherwise prominently involved in the revolutionary events. The dossiers compiled by this committee constitute the principal source from which an overall picture of the social origins of the February insurgents can be reconstructed.[2] The following analysis of these documents is intended to describe for the first time the social composition of participants in the February Revolution; to compare in general terms the February insurgents with those implicated in the other great French insurrections of the nineteenth century; to effect a more specific comparison with the insurgents of July, 1830, the principal analogous case, in order to see whether received interpretations of the February Revolution are consistent with the empirical record; and to assess whether the pattern of "artisanal activism," now well established for each of the other insurrectional movements of the period, applies in this case as well.

THE STRENGTHS AND LIMITATIONS OF THE RECORDS left by the Commission on Compensation directly affect the conclusions I offer here. The cardinal virtue of the records is their availability. Archival sources concerning Paris under the Second Republic are all too scarce, in part because municipal records were stored in the Hôtel de Ville, which burned to the ground during the last days of the Paris

[2] These dossiers are to be found in the Archives nationales de France, Series F^{1d} III (hereafter, AN). It should be noted that other potential sources exist. The archives of the prefecture of police of the department of the Seine contains a quantity of dossiers referring to recipients of national compensation (Series Aa 370–417). Most of these individuals, however, were participants in the July Days and in other conflicts, and the state of these records makes it difficult to separate out those relevant to February, 1848. In addition, the Archives nationales de France holds a set of documents gathered immediately after the February Days in preparation for the trial of Guizot and others of Louis Philippe's ministers (AN: BB^{30} 296–98). These documents are inventoried in Crémieux, *La Révolution de février*, and their origin and quality are discussed in Albert Crémieux, "Le Procès des ministres en 1848 et l'enquête judiciare sur les journées de février," *Revue d'histoire moderne et contemporaine*, 9 (1907–08): 5–23. Included among these documents are the responses of the ward mayors of Paris to a letter requesting that they provide an enumeration of those who participated, were wounded or killed in the events of February. Unfortunately, the responses were so variable in quality that we possess relatively complete lists for only a few quarters and virtually no information at all for others. Given the unevenness of the returns by quarter and the variation in average socioeconomic status between quarters, such an unsystematic data base is inappropriate for the present purpose of establishing the overall occupational distribution of participants in the February insurrection. For this reason, I have mainly relied on the compensation lists to compile the systematic data reported here and have used the information provided by the ward mayors only for confirmation.

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Commune of 1871. At the same time, these documents incorporate a number of potential biases that must be taken into account in interpreting their contents, the most obvious being that the records of the Commission on Compensation are very incomplete. A pamphlet published by Nougarède de Fayet in 1849, when the entire set of compensation records was still intact, indicates that the commission had received 7,204 claims.[3] Using a more restrictive standard for inclusion than the mere submission of a claim, my examination of the surviving records turned up just 550 files that clearly refer to participants in the February Days.[4]

From the records themselves, it is possible to reconstruct in part what the application and review process must have been. The provisional government invited applications from anyone who had suffered personal injury as a result of efforts made in behalf of the overthrow of the monarchical regime. Typically, applicants initiated the process by submitting claims detailing the roles they had played in the February Days and usually stating any circumstances that had a bearing on their financial needs. For each individual soliciting assistance, a file was opened and, after the first few weeks, classified according to whether the February combatant had been killed, wounded, or otherwise engaged in the fighting.

The worker Albert (nom de guerre of Alexandre Martin, a veteran of the secret societies and member of the provisional government) had initially been appointed to preside over the work of the commission. A decree of 2 May 1848 replaced him with Armand Marrast, mayor of Paris. The mayor set up three subcommittees to rationalize the review process. One of these was concerned with casualties and one with combatants of the February Days. The third was asked to rule on the claims of those who had suffered for the republican cause under the Orleanist monarchy. A "general committee" coordinated these activities, working closely with the Paris prefecture of police and the provincial authorities. The commission also had its own small investigative staff, which was kept busy taking depositions from witnesses to the events in which applicants were purportedly involved as well as from physicians familiar with individuals' medical records. In addition, investigators recorded testimony from employers, neighbors, fellow workers, and other interested parties concerning the background and character of those under

[3] See Auguste Nougarède de Fayet, *Les Blessés de février* (Paris, 1849). I cite this source as the best information available. I have, however, made little use of the detailed calculations and internal breakdowns that the author provided, since, despite my best efforts, I have been unable to reconcile his internally inconsistent figures.

[4] As a further indication of the incompleteness of the surviving documents, note that the remaining dossiers represent individuals whose last names begin with just fourteen letters of the alphabet. Had I been able to assume that the files of all those whose last names began with these fourteen letters had survived, it would have been possible to arrive at an independent estimate of the total number of applicants (and by extension, with allowances made for other sources of bias, to obtain a crude estimate of the number of February casualties) by extrapolating from the known distribution of French last names in the nineteenth century. In fact, a perusal of the compensation records makes it apparent that no such simple calculation is possible. Certain missing files were sent by the commission to another administrative entity for investigation or review. Some cartons contain files that do not refer to the February Days at all. (The majority of these concern the insurrection of June, 1848.) In others, one finds files that are indecipherable in whole or in part because fire has charred the contents. The coverage of names among those letters of the alphabet that are represented is irregular. It is therefore impossible to reconstruct with accuracy the national, cultural, or even occupational composition of the group as a whole.

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consideration for compensation. On occasion, the applicants themselves were interviewed by the members of the commission.

The compensation offered might be no more than honorific recognition of an individual's role in the fighting, especially in the case of an unwounded combatant or of someone who received a wound but was relatively well off. More often, however, the commission awarded monetary compensation for medical costs, lump-sum payments for those temporarily unable to work, regular support for those permanently disabled, or survivor benefits for those who had lost the livelihood provided by a son or husband who had died in the fighting. The most frequent award among those who received any money at all was 250 francs, the equivalent of two or three months' earnings for a skilled worker or as much as a year's income for an unskilled manual laborer in a trade subject to seasonal layoffs.[5] Though altogether exceptional, the largest award—24,000 francs paid over a period of twenty years to the widow of a member of the Orleanist Chamber of Deputies killed by accident on the Place de la Concorde—represented a fabulous sum for the period. Because a great deal of money was at stake, the commission appears to have pursued its inquiries with great thoroughness.[6] It was at pains to ensure that there should be no confusions of identity, no collusion between applicants and witnesses in advancing false claims, and no gross misrepresentation of financial need. While the commission may not have been perfectly successful in its verification efforts, it was more likely to have erred by denying assistance to individuals whose participation was genuine but imperfectly documented.

The files vary enormously in size and completeness, but they are typically quite rich in information about applicants and their role in the February Days. Most reveal not only where and when the applicants were born and what trades they had previously exercised but what parts they may have played in other Parisian insurrections, where they lived in February, and the precise locale and sequence of their participation in the fighting. Inquiries made at the applicants' places of work and residence often turned up evidence that concerned not just their activities at the end of February but also their political views, drinking habits, and romantic liaisons. The commission occasionally received unsolicited letters, signed and unsigned, denouncing those under consideration on grounds of fraud or misrepresentation.

It should be noted that not all "participants" in the February Days were insurgents. Sixteen files concern injuries to uninvolved bystanders; twenty-one provide insufficient information to establish the claimant's status. Curiously, thirty files, though relatively complete, remain purposefully ambiguous as to the role played by the applicants in the events of February, and nine persons (mostly municipal guardsmen) were awarded money for wounds received as part of the

[5] The median and average awards (500 and 779 francs, respectively) suggest the strong upward skew in the distribution of compensation, with a few highly visible individuals accounting for a substantial portion of all funds disbursed.

[6] According to Nougarède de Fayet in *Les Blessés de février*, 493,000 francs had been paid out by November of 1849, and perhaps as much as 876,000 francs remained in the fund accumulated from voluntary contributions after the February Days and specially earmarked for this purpose.

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unsuccessful attempt to put down the February Revolution.[7] In consequence, just 473 files could unequivocally be judged to refer to February insurgents. Unless otherwise specified, the results reported below apply solely to this group of 473.

Whether this group faithfully represents all those who assumed an active role in the February Revolution is impossible to determine, either on the basis of internal evidence or via direct comparisons with other insurrections of the period. One difficulty can be illustrated by the observation that the percentage of applicants for compensation who were long-term political activists seems high: 14.4 percent had documented histories of participation in insurrections before the February Days; 10.1 percent took part in insurrectional activities subsequent to those events. One might be inclined to suspect that individuals actively engaged in networks of revolutionary activists were more likely to learn of and act on the government's call for applications and that, as a result of such self-selection, their presence has been exaggerated. But it is also quite plausible that their involvement in such networks is precisely what makes activists quick to mobilize in an insurrectionary situation and that their numbers among applicants are a faithful reflection, conceivably even an underrepresentation, of their disproportionate involvement in the events themselves. Nothing in the files helps us to choose among these alternatives.

One might hope that comparison with other Parisian insurrections—for example, that which took place just four months later—could help to decide the question of representativeness. But, on consideration, it is obvious that any such reasoning is circular. If, to continue the example used previously, the percentages of long-term revolutionary activists were found to differ sharply between the February and June insurgents, it would nonetheless be impossible to determine whether the discrepancy resulted from valid, substantive differences between the two insurrections or from shortcomings in the data. By extension, even if the percentages were identical in the two cases, one could not be certain that this finding had not been produced by biases in the data that had the effect of masking genuine differences between the two groups of insurgents. Thus no trustworthy assessment of the representative character of these documents is possible on the basis of such straightforward comparisons.

It is not necessary, however, to abandon efforts to draw conclusions from this source. First, although this group of 473 individuals was only a subset of all February insurgents, the compensation records make it clear beyond a doubt that each was an active and authentic participant. While one must entertain the possibility that this group differed systematically from the revolutionary crowd at large—because these files represent the dead or wounded rather than the general run of participants or simply because these individuals (or their survivors) submitted a claim to the Commission on Compensation when others did not—it

[7] Awards made to individuals who acted as would-be repressors during the February Days appear to have been initiated after the reaction to the *journées* of May and June had drastically changed the political climate. The figures reported here take no account of the considerable number of files contained in cartons 84 to 97 of AN Series F^{1d} III that, as previously mentioned, refer not to February at all but to the July Days of 1830 or to the June Days of 1848.

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nonetheless constitutes a substantial group of documented insurgents and the largest such group for which systematic observations are available.

More important, problems of interpretation associated with suspected biases in the data can in part be abstracted by adopting a comparative perspective. The processes that generated these documents and allowed them to survive to this day may well have introduced unknown and unknowable distortions from the historical reality we seek to reconstruct. Yet documents of just this kind, subject to much the same distorting influences, have been previously used to describe participants in other insurrections. This was notably the case in David H. Pinkney's 1964 article in the *American Historical Review*, "The Crowd in the French Revolution of 1830." Insofar as compensation records relating to the events of July, 1830, incorporate the same types and degrees of bias as those of February, 1848, it becomes possible to bracket many of the influences that could distort efforts to reconstruct in absolute terms the collective identity of participants and to effect comparisons that are cogent because they consider only relative differences between those two insurrections.

Thus, to the legitimate questions that could be raised about the quality and representativeness of the sources relied on in this research, at least two answers are possible. The first is the more traditional response that these data, which bear on 473 individuals who could unambiguously be identified as February insurgents, represent a distinct improvement over the present lack of information. Indeed, in light of the previously discussed difficulties associated with successful insurrections as well as the sorry state of archival materials referring to February of 1848, these compensation records are the best that are likely ever to be available. Second, insofar as questions about the social identity of participants in the February Days can be cast in terms that presuppose a comparison with similar events, the problematic consequences of the data's limitations can be minimized.

THE TYPICAL FEBRUARY INSURGENT, according to this compilation of the compensation records, was a male, thirty-five years old in 1848, married, and the head of a small household. He was likely to have been born in the provinces, where he received only a rudimentary primary education, and to have come to Paris to work as an artisan in one of the skilled trades that flourished in the capital. This capsule version of the aggregate characteristics of February insurgents is accurate as far as it goes, but it conceals the sometimes wide variations among the individuals who made up that group. A closer scrutiny of their principal traits is needed, taking, wherever feasible, the participants in the June insurrection of four months later as the point of comparison.

Although a good many applicants for compensation were the female survivors or dependents of those injured in the fighting, when consideration is limited to those who could be identified with certainty as February insurgents, virtually all

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TABLE 1
Age Distribution of February Insurgents

Age in Years	*N*	*Percent*
10–19	38	9.1
20–29	98	23.6
30–39	137	32.9
40–49	102	24.5
50–59	30	7.3
60–69	8	1.9
70–77	3	0.7
Total	416	100.0

are found to be males.[8] This is true to an extent slightly greater even than in the case of their June counterparts: 98.9 versus 97.6 percent.[9] With a mean age of 35.3 years, February insurgents were nearly two years older on average than those who participated in the June Days.[10] The range of ages encountered in the February compensation records extends from fourteen to seventy-seven years. As Table 1 indicates, the repartition of ages tapers fairly evenly in both directions and describes a nearly normal distribution around the average. Roughly one-third (32.9 percent) of all insurgents were in their thirties, with one-third (32.7 percent) younger and one-third (34.4 percent) older.

This convenient three-way division makes it relatively easy to translate these age groupings into the distinct socializing events and experiences to which their members had been exposed. The modal cohort, in its thirties at the time of the

[8] All findings concerning "February insurgents" below refer solely to individuals whose compensation records were sufficiently detailed and complete to establish unambiguously two facts about them: that they were active in the February Days and sided with the forces of insurrection. The main categories thus eliminated are: beneficiaries who were not themselves insurgents; those making claims for compensation based on injuries or persecutions suffered in the cause of the Republic either prior or subsequent to the February Days (mainly June, 1848); bystanders; and, in a few cases, those who were wounded during the February Days as part of the attempt to crush the insurrection.

[9] Unless otherwise specified, data in this section that refer to "June insurgents" are drawn from Lynn H. Lees and Charles Tilly, "Analysis of Arrests in Paris: June, 1848," Codebook obtained through the Inter-university Consortium for Political and Social Research (Ann Arbor, Mich., n.d.); and from Charles Tilly and Lynn H. Lees, "The People of June, 1848," in Roger Price, ed., *Revolution and Reaction: 1848 and the Second French Republic* (New York, 1975), 170–209. Although I consider the research by Tilly and Lees to be the most systematic and the easiest to use with regard to the June Days, the article by Pierre Caspard, "Aspects de la lutte des classes en 1848: Le Recrutement de la garde nationale mobile," *Revue historique*, 511 (July–September 1974): 81–106; and the book by Roger Price, *The French Second Republic: A Social History* (Ithaca, N.Y., 1972), should also be recognized as important alternative sources. For purposes of strict comparability with Tilly and Lees's data, it would be best to eliminate from my February data insurgents who were not residents of the twelve wards of the city of Paris. This would, however, make the February data less comparable with Pinkney's figures on the insurgents of July, 1830, discussed below. Rather than introduce inevitable confusions by presenting two similar but distinct sets of February data, I have chosen to consider the entire group of February insurgents, verifying in every instance that differences between Paris residents and suburban residents are insignificantly small.

[10] Lees and Tilly reported that June insurgents had an average age of 33.4 years. Caspard's corresponding figure was thirty-four years.

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February Revolution, had been born roughly at the close of the empire. It spent its childhood under the first decade of the Bourbon Restoration. In its key formative years, it experienced first the July Revolution (as teenagers, for the most part) and then (as its members approached majority) the sequential insurrections—1832, 1834, and 1839—that marked the early years of the Orleanist monarchy. Yet this enumeration of the turbulent collective experiences of the members defies extrapolation into an explanation of the February Days, since fully one-third of all insurgents were older, many of them growing up under the glorious years of Napoleon's empire; while another third were younger and had been shaped under the later and more stable years of Louis Philippe's rule.

When one considers place of birth, a pattern fairly typical of the demographics of mid-nineteenth-century Paris emerges. While the department of the Seine was far and away the single most heavily represented place of birth, it still accounted for only a minority (28.2 percent) of all Parisian insurgents. No fewer than sixty-seven of the eighty-nine French departments were represented among the 355 applicants who could be coded on this variable, a diversity of geographical origins that was no more than the norm among inhabitants of the capital in that period. Similarly, 5.1 percent of all February insurgents were foreign-born, a figure that compares with Lees and Tilly's report that 5.4 percent of June rebels were born outside France. The percentage of February and June insurgents who were married was almost identical: 58.8 versus 59.4 percent, respectively. And although no adequate measure of literacy was available, applicants were asked to characterize their level of instruction.[11] An absolute majority (69.4 percent) described themselves as possessing a primary education, whereas just 17.9 percent said they had received advanced instruction.

Yet the finding of greatest interest—particularly to social historians—concerns the occupational status of February insurgents. According to the applications submitted to the Commission on Compensation, participants in the February Days were artisans in overwhelming proportions, plying the full range of skilled trades that constituted the core of the Parisian economy. Of course, the distribution of self-identified occupations cannot in any literal sense be construed as a measure of social class. It actually places an individual "horizontally" within an occupational group or economic sector rather than "vertically" within a hierarchy of social status. But this is no more than an accurate reflection of the subjective reality of most workers in the mid-nineteenth century, who typically thought of themselves as members of a trade and identified primarily with others practicing the same craft, whether higher or lower in status, rather than with their status equals in different branches of the urban economy.

[11] Despite serious reservations, I would have been willing to accept as a criterion of literacy the individual's ability to sign his or her own name. This standard has, after all, consistently been employed for this purpose in similar research and therefore has value in making comparative assessments, even though it is a crude indicator of the ability to read or write. I was unable to effect even this imperfect comparison because the application process involved no specific occasion requiring that the claimant sign his or her name, and therefore the absence of any document bearing a signature remained uninterpretable.

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TABLE 2
Sector Distribution of February Insurgents

	*All Paris**		*February Insurgents*†*	
Sector	*Percent*	*Rank*	*Percent*	*Rank*
Food	3.9	10	2.2	10
Construction	19.6	1	25.4	1
Furniture	14.0	3	9.9	4
Clothing	14.8	2	18.1	2
Textiles	5.4	6	6.0	6.5
Leather	2.1	12	1.3	11
Carriages	5.2	9	3.0	9
Chemicals	3.2	11	0.4	12.5
Base Metals	10.8	4	13.8	3
Precious Metals	5.3	7.5	8.2	5
Cooperage	2.0	13	0.4	12.5
Fancy Goods	8.6	5	5.2	8
Printing	5.3	7.5	6.0	6.5
Total	100.2		100.0	
(N)	(204,925)		(232)	

* Source is the Chambre de Commerce de Paris, *Statistique de l'industrie à Paris* (Paris, 1851). Because virtually all February insurgents were males, I have included only males in calculating the sectoral distribution of the Parisian labor force.
† For the sake of comparability with the Parisian data, I have included only those February insurgents who were residents of the city proper.

By grouping individuals into the thirteen sectoral categories used by the Paris Chamber of Commerce in its 1848 survey of those working in the production of goods, it is possible to compare the February insurgents with the general working population of the capital. Table 2 displays the distributions by sector for both groups, as well as their rank orders. A comparison of each sector's pair of rank orderings clearly shows that, despite a number of minor discrepancies, the sectoral distribution of February insurgents was broadly representative of the Parisian working class as a whole.

These same data can be used to calculate an "index of participation" by dividing each sector's share of February insurgents by its share of the general Parisian labor force and the index used as a gauge of over or underrepresentation in that conflict. The results (Table 3) indicate that workers in metal trades (both "precious" and "base"), construction, clothing, printing, and textiles were more heavily concentrated among the 1848 revolutionaries than in the population at large. Although these findings should be viewed with caution because of the relatively modest size of the sample of February insurgents, it is worth remarking that radical action in

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TABLE 3
Sector Representation in the February Days

Sector	*Index of Participation**
Food	56.4
Construction	129.6
Furniture	70.7
Clothing	122.3
Textiles	111.1
Leather	61.9
Carriages	57.7
Chemicals	12.5
Base Metals	127.8
Precious Metals	154.7
Cooperage	20.0
Fancy Goods	60.5
Printing	113.2

* The index of participation was calculated by multiplying by 100 the quotient of a sector's percentage representation among February insurgents and its representation among male Parisian workers according to the Chambre de Commerce, *Statistique de l'industrie à Paris*. Thus a value of 100 indicates precisely proportional representation, while values higher or lower indicate relative over or underrepresentation in insurgent activities.

the February Days appears to have been dominated by many of the same groups traditionally associated with revolutionary activity in this period.[12]

THE MOST INSTRUCTIVE COMPARATIVE CASE against which to assess the February Days is the July Revolution of 1830. Both sets of events involved large-scale uprisings that toppled the regime in power with the apparently unanimous support of the Parisian population. Because both were successful insurrections, compensation records constitute the only systematic documentation of participation left behind. The published research of David H. Pinkney, based on applications for compensation resulting from the July Days, makes it possible to

[12] A second observation, which anticipates the mention of the "thesis of artisanal activism" made in the conclusion below, seems in order. While the results just reported are quite compatible with the consistent finding of social historians investigating French insurrections of the nineteenth century, that highly skilled artisans predominate overall, workers drawn from the most proletarianized sectors—clothing and, to a lesser extent, textiles—were also overrepresented. Thus one might argue that it is as much the state of development of the urban economy as it is a condition specific to a sector or trade that is associated with participation in insurrections. In this view, artisans are prominent in such events because they happen to be numerically predominant during a phase of economic and political development in which revolutionism is widespread. For an intriguing theoretical exploration of such a hypothesis, as yet neglected as a subject of systematic empirical research, see Gareth Stedman Jones, "The Mid-Century Crisis and the 1848 Revolutions: The Case of England," in Edmund Burke, ed., *Global Crisis and Social Movements: Artisans, Peasants, Populists, and the World Economy* (Boulder, Colo., 1987), 72–85.

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effect a comparison with the February Revolution in general terms.[13] This juxtaposition shows, for example, that females, even though three times more heavily represented in July, nonetheless remained few in absolute numbers. Males represented 96.6 percent of applicants in 1830 as compared to 98.9 in 1848. Although data on age are too incomplete to quantify in comparable form, it also appears that February insurgents were appreciably older than their July counterparts.[14] Insurgents in both periods appear to have been drawn from provincial departments in identical proportions (71.7 versus 71.8 percent).[15]

The general picture of the characteristics of insurgents in these two periods that emerges from this comparison is one of remarkable similarity, despite considerable variation in detail. Yet it is the question of insurgents' occupational status that remains of paramount interest. Here again, we are handicapped by the form in which Pinkney reported his findings. He did not publish an inclusive summary of the occupations of all individuals who provided this information. Instead, without spelling out the rationale for his choices, he selected categories that refer variously to specific trades or to whole sectors of the economy, posing nearly insoluble

[13] See David Pinkney, "The Crowd in the French Revolution of 1830," *AHR*, 70 (October 1964): 1–17. This article was also published, with minor changes, as a chapter in David H. Pinkney, *The French Revolution of 1830* (Princeton, N.J., 1972). Unfortunately, neither of these sources fully describes Pinkney's data base or provides inclusive information for the entire sample on the most important variables. In a personal communication, 21 December 1987, Professor Pinkney informed me that the raw data are no longer available. To make comparisons as useful as possible within the limitations of the existing data sets, it has therefore been necessary to adapt my data on February of 1848 to the format applied in the case of July of 1830, with some consequent loss in richness.

[14] Problems arise because Pinkney reports in "The Crowd" only that 54 percent of July insurgents were between the ages of twenty and thirty-five years. This figure is based on lists of those who died and of those wounded. In the February Days, the corresponding figure was but 44.5 percent. Because of Pinkney's elliptical form of reporting, it is technically impossible to say on which side of this cohort the bulk of the remaining participants fell with respect to age and therefore which group was older overall. Some help is offered by a 1972 article Pinkney published comparing the insurrections of 1830, 1832, 1834, and 1839. (See David H. Pinkney, "The Revolutionary Crowd in Paris in the 1830s," *Journal of Social History*, 5 [Summer 1972]: 512–20.) Although those data are also incomplete, are based only on those killed during the July Days, and use inconsistent methods of aggregation, this source describes an age distribution that makes it virtually certain that the revolutionaries of 1830 were very significantly younger than their counterparts in the February insurrection. Indeed, the July Revolution appears to have attracted participants not only younger than those involved in any other broad-based and spontaneous insurrection in France of the nineteenth century but younger even than the militants who took part in most of the smaller and more organized uprisings of the 1830s. One possible line of explanation lies in the observation that the July Revolution of 1830 occurred near the onset of a twenty-year period that witnessed frequent and intense bursts of insurrectionary activity. The hypothesis that revolution constitutes a "career" of sorts, and that 1830 therefore represents the apprenticeship of a cadre of insurrectionary participants who would turn up repeatedly in arrest and compensation records as they gradually aged, seems worthy of further investigation. For present purposes, however, it remains impossible to calculate comparative age distributions in a systematic fashion without returning to the original sources.

[15] In his 1964 *AHR* article, Pinkney actually reported his figures disaggregated into two categories: those who died and those who were wounded or combatants. I have reaggregated the figures based on the sample sizes that he reported for these two groups. It should be noted, however, that the statistical identity between July and February insurgents on this variable may conceal significant substantive differences, since the age distributions of the two groups are known to be somewhat discrepant, and in-migration from the provinces is known to be age-related and to have varied over time. However, discrepancies in the form of these two data sets make it impossible to disentangle these more complex relationships.

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TABLE 4
Insurgent Occupational Distribution (Percent)

Sector	*July 1830**	*February 1848*
Carpenters, joiners, cabinetmakers	8.2	11.8
Masons	7.7	2.9
Tailors	1.8	3.8
Shoemakers	6.1	5.2
Locksmiths	3.7	2.3
Jewellers	2.0	3.4
Printers	1.8	1.1
Professionals	5.5	2.9
Students	0.7	1.1
Clerks	5.1	7.2
Shopkeepers	3.5	2.7
Total	46.1†	44.4†
(N)	(1,538)	(442)

* Source: Pinkney, "The Crowd in the French Revolution of 1830."
† These figures do not sum to 100 percent because Pinkney did not specify the occupations of most of those he coded, in particular the approximately 500 individuals he says were artisans working outside the trades he enumerated.

difficulties for systematic comparison.[16] My only recourse has been to abandon the classification by sector of the economy that has proved to be such a useful standard in work comparing the other insurrections of the mid-nineteenth century. Instead, I have assembled all statements made by Pinkney that specify the number of practitioners of a trade, grouping of trades, or broader occupational category, thus permitting me to calculate the percentage each represented of all July insurgents. I was then able to aggregate or disaggregate my own data on February, 1848, into categories that correspond as closely as possible to each of Pinkney's statements. The result is the occupational distribution displayed in Table 4. A number of discrepancies become immediately apparent,[17] but, in general, the percentages

[16] Pinkney sometimes aggregated several trades that, according to traditional systems of categorization, belong to different sectors. In addition, he often phrased his enumerations in vague terms ("fewer than 300 were laborers and servants"; "nearly 1,000 were artisans and skilled workers"). See Pinkney, "The Crowd," 3.

[17] These variations are not entirely surprising in light of the now further restricted size of the February sample (only 196 individuals are represented in the second column of Table 4) and the manipulations to which the data were subjected in order to make them suitable for comparison. The most striking is perhaps the relative overrepresentation of masons in the July Days. Although Pinkney himself notes that their rate of participation in those events was more than double what one might expect, given their prevalence among adult Parisian males, it should also be kept in mind that their representation among applicants for compensation resulting from the February Days corresponds to a modest (and therefore statistically volatile) thirteen individuals.

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describe a pattern sufficiently similar in the two cases to justify the conclusion that both sets of insurgents were drawn from much the same strata of society.

The total for all trades enumerated by Pinkney for July, taken as a percentage of all insurgents (46.1 percent), ends up remarkably close to the corresponding figure (44.4 percent) for the same categories in the February Days. Pinkney also mentioned that "nearly 1,000" (that is, 65 percent) of all 1830 revolutionaries were artisans. For February of 1848, the sum of those in the thirteen artisanal sectors comes to 60.9 percent, and, if one adds workers in the transport and service sectors (for the precise limits of Pinkney's definition of an artisan are ambiguous), the cumulative percentage rises to 69.0 percent.

Thus, the juxtaposition of these most comparable of all nineteenth-century Parisian insurrections shows that the February Revolution of 1848 was no more the work of an urban underclass of dispossessed and socially marginal elements than was the July Revolution of 1830. Instead, a broad coalition of skilled workers was joined by a small but appreciable admixture of members of the commercial and professional classes. Pinkney has called attention to the false impression created by Eugène Delacroix when he placed a female figure, flanked by a top-hatted bourgeois and a pre-adolescent street urchin in the center of his "Liberty Guiding the People." The reality in the July and February Days alike was both more prosaic and more consistent with what we have learned in the past two decades about the other insurrections of the nineteenth century: the great bulk of the fighting, and an even greater share of the loss of blood and life, was borne by members of the laboring classes, specifically by the highly skilled and relatively privileged artisans who predominated in the Parisian labor force.

A STATISTICAL ABSTRACT OF THE FEBRUARY INSURGENTS OF 1848 thus presents strong parallels with July of 1830. But the similarities do not end there. Pinkney stipulated the main precipitating factors in the July Revolution to have been the rekindling of popular resentment against the Bourbons by the publication of the five ordinances; the appointment of the unpopular General Marmont, duke of Ragusa, to command the Paris garrison; and a sequence of symbolic events including the reappearance in the capital of the tricolor flag and the ringing of the bell of Notre Dame. These had their precise counterparts at the time of the February Days in the revival of popular resentment against the Orleanist monarchy, seen by republicans and populists as having "stolen" the Revolution of 1830, in consequence of the government's resolve to prevent the democratic banquet of 22 February; in the appointment of the hated General Bugeaud to head the repression of the incipient insurrection; and in the aftermath of the massacre of the Boulevard des Capucines, when the bodies of the dead, displayed by torchlight on an open wagon, were paraded through the city.

Just as significant are the parallels to be found in the preconditions of the two insurrections. Pinkney noted the serious economic difficulties that had afflicted France in the period immediately preceding the July Revolution. These began with poor harvests in 1827 and 1828, precipitously raising the price of bread. Crisis in

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the agricultural sector soon spread to the commercial and industrial sectors, as unemployment rose and wage levels fell. In much the same fashion, the February Revolution was heralded by disastrous harvests in 1845 and 1846, triggering a serial crisis that spread from the agricultural to the commercial and industrial sectors and resulted in economic dislocations of even greater magnitude than in the 1820s. Thus the pattern of the "old-style" crisis described by Ernest Labrousse in his classic 1948 article appears to link the two periods through their shared sequence of material causes.[18]

Still, Pinkney rejected the notion that economic distress was the principal driving force behind the July Revolution. He noted that the Parisian economy had already recovered to an appreciable degree in the year preceding the outbreak of the July Days. Here, too, one finds on examination that much the same holds true of the mid-century crisis, which had considerably eased just before the February Revolution itself plunged the French economy into the deepest depression of the modern era.

But Pinkney's main reason for ruling out economic distress as the direct cause of the fall of the Orleanist monarchy is the fact that July combatants were primarily drawn not from the poorest segments of the Parisian population but from the most skilled and privileged strata of the working class, who were not at that time particularly affected by unemployment. In this respect, too, they resembled the insurgents of February who were for the most part the same kind of skilled artisans who predominated in the Parisian economy at large and who enjoyed a slight, if only relative, margin in the struggle for existence over the most poverty-stricken fraction of the lower class.

This crucial parallel between the social identity of participants in the February Revolution of 1848 and in that of July, 1830, also extends—as the research of Rudé, Tilly and Lees, and Rougerie has shown us—back to the French Revolution and forward to the June Days and the Paris Commune.[19] All fit the pattern of "artisanal activism" and so raise serious questions about explanations based strictly on economic determinants. This is not to deny either the real variations in objectives and tactics that separate the major insurrections of the nineteenth century or the role of material considerations in a February Revolution whose key demands included the right to work and the organization of labor. Yet evidence from the February compensation records requires an explanation based on more than economic deprivation, even as it confirms this basic finding of the social history of the past two decades: that the faces in the revolutionary crowd in France changed little between 1789 and 1871.

[18] See Ernest Labrousse, "1848, 1830, 1789: Comment naissent les Révolutions," in Comité français des sciences historiques, *Actes du Congrès historique du centenaire de la révolution de 1848* (Paris, 1948), 1–29.

[19] The key works I have in mind are George Rudé, *The Crowd in the French Revolution* (New York, 1973); Tilly and Lees, "The People of June, 1848"; and Jacques Rougerie, "Composition d'une population insurgée: L'Exemple de la Commune," *Le Mouvement social* (July–September 1964): 31–47.

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Review Article

What Women Have Wrought

MARJORIE MURPHY

Susan Porter Benson, **Counter Cultures: Saleswomen, Managers, and Customers in American Department Stores, 1890–1940** (Urbana: University of Illinois Press, 1988). Pp. 334, $27.50, paper 10.95.

Patricia A. Cooper, **Once a Cigar Maker: Men, Women, and Work Culture in American Cigar Factories, 1900–1919** (Urbana: University of Illinois Press, 1987). Pp. xvi, 350, $29.95.

Carol Groneman and Mary Beth Norton, eds., **"To Toil the Livelong Day": America's Women at Work, 1780–1980**, Essays from the Sixth Berkshire Conference on the History of Women (Ithaca: Cornell University Press, 1987). Pp. xi, 312, $34.95, paper 9.95.

Ruth Milkman, **Gender at Work: The Dynamics of Job Segregation by Sex during World War II** (Urbana: University of Illinois Press, 1987). Pp. xv, 213, $32.50.

CULTURAL STEREOTYPES ABOUT THE NATURE OF WORK AND WOMEN are so persistent in the history of working women that the wide range of women's experiences as laboring people is often ignored in favor of stereotypical expectations. The most pervasive such assumption has been that women avoid high-pressure or strenuous jobs and are unwilling to take risks in advancing their own interests on the job. Only recently have we begun to break away from these expectations to uncover a rich and rewarding history. Yet recurrent themes continue to plague the literature. Although women's work has changed across time and has differed among economic, ethnic, racial, and age groups, the difficulty of combining wage earning and domestic work has remained remarkably constant. This continuity contributes to a sense of timelessness, robbing working women of historicity. Consider, for example, Henry Ford's dictum cited in one of the books reviewed here: "The man does the work in the shop, but his wife does the work in the home." Yet Ford lived in an era when women entered the paid labor force in millions, when women stayed in the work force after marriage, and when world wars brought women into the same jobs that male auto workers occupied.[1] Still, Ford believed that women who worked did so "to buy fancy clothes."

Assumptions about the nature of women have also led to restrictions on the work available to women; these restrictions shaped the contours of the labor market and the choices held open to women. The resulting historical shape of the female labor

[1] Lynn Weiner, *From Working Girl to Working Mother: The Female Labor Force in the U.S., 1820–1980* (Chapel Hill, N.C., 1985), 84–89.

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market defies conventional notions of the invisible hand of the market system. As they did in the past, women work—in low-paying jobs in domestic service, clerical work, or unskilled manufacturing—not because they prefer to do so but because these employers prefer to hire women. Within industries, sex segregation further defines roles for women, often shaping relationships between men and women on the job and in trade unions. Even in the study of labor history, cultural stereotypes leave an insidious mark on the field, and gender analysis is seen as an alternative to rather than an augmentation of class analysis. The predominance of women in low-paying, non-industrial jobs, the segregation of the sexes within industrial enterprises, and the persistence among labor historians of conventional notions about the place of women all perpetuate cultural stereotypes.[2] Against these stereotypes, a formidable array of scholarship has begun its testimony.

The first of the four books considered in this review illustrates how industrialists used cultural stereotypes to define a two-tiered pay scale providing maximum flexibility for management and minimum wages for women. Ruth Milkman has written with superb insight into the extensive sex segregation of the auto and electrical industries. She shows that sex labeling, or the assigning of characteristics of gender to achieve sex segregation, was a pervasive tool used in industry to divide male and female workers. The presence of women workers kept wages low, while the possibility of female substitution threatened male job security. Electrical manufacturers located in cities, where wage rates were low and where large numbers of women and girls would work for piece-rates that were lower than the rates paid to men. In automobile manufacturing, high wages for men were key to controlling labor. Because of the family wage, women were excluded from the five-dollar day and allowed to work only in a handful of segregated shops, while seasonal unemployment kept men workers cognizant of their superior wage and the army of unemployed willing to replace them. Sex labeling was achieved not by reference to women's family role but rather to their real or imagined physical characteristics. Women were thought to be better suited to "light" assembly jobs: upholstery units in auto making, delicate and monotonous assembly work in the lamp industry. When wartime industry needed women in men's jobs, recruiters made analogies to domestic chores such as "the similarity between squeezing orange juice and the operation of a small drill press." Milkman illustrates that sex labeling was inconsistent both over time and within industries. Women crane operators were thought to be more sensitive and accurate at Hudson Motor Company, whereas elsewhere in the auto industry running a crane was considered a man's job. The doctrine of equal pay for equal work challenged this system in a limited way but often precluded the equalization of work between men and women.[3]

[2] See Joan W. Scott, "On Language, Gender, and Working-Class History," *International Labor and Working-Class History*, 31 (Spring 1987): 1–13; and responses by Bryan Palmer, Christine Stansell, and Anson Rabenbach; Joan W. Scott, "A Reply to Criticism," in *International Labor and Working-Class History*, 32 (Fall 1987): 39–45.

[3] Milkman, *Gender at Work*, 16, 19, 61.

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Milkman's study, although focused on World War II, speaks broadly to issues of twentieth-century labor in two key expanding industries, electrical and auto, that were scenes of the greatest triumphs for the Congress of Industrial Organizations (CIO). The confusion about "men's jobs" and "women's jobs" was predictably compounded during the war and resulted in a number of postwar shuffles, which Milkman expertly untangles to reveal managerial hostility toward female employees, especially in the auto industry. Milkman broadens our definition of Fordism to include the assertion that women were just not suitable for manufacturing work, and she emphasizes that management's goal was not simply to reduce pay levels but, more important, to assert control over labor. While she demonstrates that men acquiesced in sex labeling when it was to their advantage, she also argues convincingly that management manipulated these sentiments to serve corporate expectations.

World War II brought more women into industrial work and, Milkman tells us, into the labor movement, notably the CIO, where female unionization grew disproportionately. This growth did not necessarily give women more power within unions. Indeed, most women were enlisted automatically; they reaped effortlessly the rewards of the CIO's past struggles, and they often maintained anti-union biases. Aware of the lack of commitment among new recruits, the United Electrical Workers (UE) and United Auto Workers (UAW) invested heavily in education programs and sought to promote women union leaders. Although strong women leaders were found, Milkman points out that they were welcomed only on men's terms. The grievances they pursued were restricted to issues that benefited the union as a whole; women organizers often put issues of class above issues of gender to preserve unity with the overpowering male leadership.

Demobilization at war's end tested every weakness of this male-female coalition. Factory reconversion brought with it a return of traditional sex segregation, which had subsided with wartime female substitution. Milkman demonstrates that management sensed ambivalence about the CIO's equal-pay-for-equal-work struggles among male workers and restored the pre-war division of labor. She challenges D'Ann Campbell's conclusions that family roles were central to women's lives in the era of reconversion and that the call to domesticity powerfully pulled women away from factory labor. Using surveys and polls, Milkman argues that women expressed more than a casual interest in maintaining their high-paying jobs. At the same time, Milkman concedes that women were being lured home by the cult of the lady, but she adds to that formula the important element that women were being pushed by the not-so-attractive sex-typed jobs they were offered.[4]

The most delicate issue in the reconversion is the question of labor's complicity in management's policy of reestablishing old sex divisions. Seniority, Milkman concludes, was rarely the culprit in the disputes between men and women. The source of discontent came from the policy of management favoring high turnover rates and its deliberate assertion of control through the re-designation of men's

[4] D'Ann Campbell, *Women at War with America: Private Lives in a Patriotic Era* (Cambridge, Mass., 1984); Milkman, *Gender at Work*, 101–02.

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and women's jobs. Unions, particularly UE and UAW, had nondiscriminatory policies, but management evaded these by replacing women with men in certain departments regardless of seniority or by downgrading women to women's jobs according to seniority. Hiring policy, not seniority, proved more detrimental to women's advancement in postwar industry. Finally, Milkman argues, although familial considerations were not explicitly articulated in managerial policy, labor relations were. Management assumed that by reverting to the old division of labor it could minimize conflict with unions.

Milkman draws our attention to sex segregation as a significant reality in twentieth-century working life. "In 1981 women were more than 99 percent of all secretaries, but less than 1 percent of all automobile mechanics; 97 percent of all registered nurses, but only 4 percent of all engineers," she tells us. "That secretaries and nurses are and should be female and that truck drivers and construction workers are and should be male continues to be the unexamined presumption of employers as well as most working people themselves."[5] Ideology in sex typing continues to dominate our habits of work and the relations of gender.

THE CHALLENGE TO WRITE WORKING-CLASS HISTORY that is knowledgeable about gender norms of the time and yet conscious of the meaning of work cultures when seen through the prism of gender has been met by Patricia A. Cooper's history of the cigar makers' union.[6] Cooper demonstrates that skilled male pieceworkers developed a special sociability on the shop floor and that it followed them after work into the saloon. In contrast, female cigar pieceworkers worked in teams, which led to conflict and at the same time unity on the shop floor. Cooper identifies different work cultures emerging from these shop settings for men and women. Outside of the shop, these work culture differences were expressed in contrasting forms of leisure: the men congregated in the saloon, the women gathered at church picnics or walked to the local park in the evenings. While Cooper does not diminish the significant advantages for men of their superior wage, their ability to travel from job to job, and the fraternity of the saloon, she insists that men and women cigar makers operated in equivalent ways to protect their territoriality on the work site. In both cases, workers employed gender to define and protect dignity on the job. Within a sex-segregated industry, it seems work cultures defined by gender both protected workers and ultimately paved the way for the downfall of the proud cigar makers' union.

Work culture nurtured solidarity but also fostered exclusivity. The strength of male cigar makers was also their weakness. Unity and fraternity protected them from the whims of management while the preservation of male privilege divided them from female workers. Cigar makers took pride in presenting a "manly" demeanor to management; they dressed formally when meeting bosses but never worked to please the boss. Ironically, the male work culture, while it defended workers' rights, provided at the same time the barrier between men and women

[5] Milkman, *Gender at Work*, 157.
[6] Cooper, *Once a Cigar Maker*.

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that assured the employers a permanently divided work force. Women cigar makers were very different from the men in the industry. Unmarried, younger, and from more recent immigrant groups, they did not join the Cigar Makers' International Union (CMIU) nor were they welcome. "Women could hardly be manly," Cooper observes.[7] In some cases, women refused to join the union because they suspected the sincerity of the men and objected to local meetings in a saloon.

According to Cooper, gender dominated the creation of a work culture for women. Because gender relations were so uneven, conditions for men and women varied greatly. Unlike the men, women were expected to observe strict work schedules; unlike the men, piecework tended to drive the women to produce at a frantic pace; and unlike the men, women worked in teams rather than depending on individual skill. Women created the same sort of distance from their employers as men; they could not be manly, but they could take advantage of distance. According to Cooper, gender brought female workers their sense of identity: "Gender defined a common experience among women cigar makers whether from the paternalism of employers, the concerns of women's shop floor conversation, or a sense of sexual solidarity vis-à-vis male managers."[8] Women did not challenge employers at every juncture, Cooper points out, but neither did the men. Employing George Rudé's notion of an "inherent ideology" to explain moments of accommodation and consent, Cooper suggests that what happened on the shop floor produced effects that were at the same time progressive and reactionary.

Cooper tries to link the peculiar work cultures of men and women to their political behavior. Collectivist, anti-capitalist behavior was not always matched in politics outside the shop; despite a strong socialist faction, the CMIU had a sizable conservative constituency. At the same time, she finds that women could be militant, especially against the new rolling machines that threatened their jobs. With these caveats in mind, Cooper recounts a tale of tragic division between men and women cigar makers as the change to machine production threatened to destroy preciously guarded rights and privileges.

Cooper's history is that of a relationship between men and women and between workers and their work world. Because it is a relationship, Cooper takes special care to delineate the various tensions and contradictions. She goes beyond cultural stereotypes to explore what is most painful in the history of the oppressed. She is unafraid of the contradictions that so bitterly divided workers. By exposing the divisions between workers, she forces us to recognize the achievement of solidarity. The strikes and struggles of the cigar makers were marked by arrogance and understanding, "privilege and equality," "resignation and resistance."[9]

Sex segregation and cultural stereotypes kept women out of certain industries, but they shaped the relations between men and women within industries as well. Clerical work clearly remains a prime example of segregated employment.

[7] Cooper, *Once a Cigar Maker*, 220.
[8] Cooper, *Once a Cigar Maker*, 238.
[9] Cooper, *Once a Cigar Maker*, 329.

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Although several new studies on clerical workers are still in progress, the field has broadened to include department store clerks in the pioneering work of Susan Porter Benson.[10] Benson calls for a new framework for understanding the dynamics of gender and class in the service industry. It seems that saleswomen and customers could join in opposing managers' attempts to cut costs; managers and customers could unite to undermine saleswomen; or saleswomen and management could act to curtail customers' excessive demands. But the logic of the new consumerism demanded too many compromises with male dominance, beginning with attempts to lure female shoppers to the stores. Benson's skillful portrait of department stores reveals that the struggle between managers and workers in the dynamic of consumer culture produced a unique relationship weakening the power of male managers to control the interactions between saleswomen and customers. Gender and class operated in a world removed from a simple profit system.

In the department store, desire was manufactured and services provided to meet that created desire; store clerks catered to but were also captured by this manufactured desire. Managers had no direct control over customer-clerk interactions. Male managers depended on empathy among women to promote sales; they relinquished absolute control while encouraging bonding between customer and saleswoman. Benson presents a world of counterbalanced demands in which the decentralization of selling characterized the relationships of workers and managers alike. By focusing on the structure of the industry, Benson gives us a rich portrait of the nature of control. By concentrating on gender relations within that structure, she uncovers issues that the feminization question only narrowly addressed. Finally, by insisting on viewing work culture as a determinant in workers' consciousness, Benson forces us to look critically at studies of women's work culture.[11]

Rejecting the dichotomy between work consciousness and women's consciousness, Benson argues that women developed skills of social interaction that, although devalued in society, were useful in department store selling. "Department store saleswomen's work culture reflected a consciousness of themselves as workers, as women and as consumers, reflecting the complexities and contradictions of their lives."[12] Work culture was, in Benson's view, "a flexible resource." Saleswomen could, through the use of the salesclerks' huddle, reprimand male workers for sexual harassment in a manner Benson equates to the power of skilled male workers on the shop floor. At the same time, salesclerks celebrated romance and promotion with equal interest. Customers could be friends, potential mates, and objects of control. These complexities and contradictions, Benson admits, "obscured and transformed the content of class conflict on the selling floor. It was not always clear who the enemy was." Consequently, although saleswomen could

[10] Benson, *Counter Cultures*.
[11] Richard Wightman Fox and T. J. Jackson Lears, eds., *The Culture of Consumption: Critical Essays in American History, 1880–1980* (New York, 1983), 1–38.
[12] Benson, *Counter Cultures*, 9.

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be militant and class-conscious, they could never launch a sustained trade union effort in their industry.

Benson relies heavily on a definition of work culture that broadens the labor historian's view of how a work culture evolves. Using the work of Barbara Melosh, Benson argues that women derived some power in sex-segregated work.[13] Accommodation to the authority of the machine or the manager alone was not the only option open to women workers in service industries. In selling, as in nursing or school teaching, a third element enters the equation: the client. The presence of this third party between manager and worker gave women an opportunity to insert their own authority. Women were hired because managers thought their special feminine characteristics would improve service. Benson further argues that the dichotomy between work culture and family culture is misleading: it distorts the ways in which women integrated their self-perceptions as women, as workers, and as consumers.

Although manliness was as inappropriate for saleswomen as it was for women cigar makers, Benson contends that women found ways to challenge authority, to separate themselves from the outlook of their bosses, and to maintain dignity. Forced to become consumer authorities, saleswomen wielded this skill as a weapon to maintain their autonomy in dress. Like the team workers in the cigar industry, saleswomen worked in close quarters within their departments, where their solidarity proved remarkably strong. The department came to represent a form of turf to be protected against arbitrary management decisions. Unique to the selling culture was the opportunity for the saleswoman to exploit her ambiguous relationship with her customer and manipulate management's desire to have her use her abilities in social interaction to please the customer. In Benson's view, the managers used cultural stereotypes to manipulate women customers and clerks, but, once deployed, this strategy of control became a way for women workers to assert the same shop floor controls that skilled men had assumed through the strategy of manliness.

The remarkable aspect of these new histories lies in the portrait of resilient women who operated against cultural stereotypes while maintaining their own working-class standards of behavior. Union women, angered by the failure of the unions to protect their jobs after World War II, tried, as Milkman tells us, to force the issue within their organizations without breaking them apart. Cigar-making women participated in militant strike actions to protect their jobs. Even the department store clerks, who were daily tempted by middle-class desires, fought managers to maintain their standards. Each of these major revisions to the history of gender and work offers a new lens for exploring the consequences of cultural stereotypes and the forces that operated against them. Employers, Milkman and Benson observe, well understood the possibilities of manipulating sex segregation to enhance their control, but gender relations were difficult to manage. As Cooper confirms, women workers could use their gender identity to distance themselves from their bosses.

[13] Melosh, *"The Physician's Hand": Work Culture and Conflict in American Nursing* (Philadelphia, 1982).

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Now that historians are moving away from the rhetoric of downtrodden working women, the puzzling issue in the history of women and work is why women who were capable of distancing themselves from male managers had such difficulty organizing unions. Trade unionism in the historiography of women and work has been confounded equally by issues of gender, consciousness, and work culture. Barbara Wertheimer's laudatory *We Were There* established women's presence in the labor movement, but it was not until Alice Kessler-Harris published *Out to Work* that an analytical approach to women in the trade unions emerged, raising new questions about the role of working women in the unions.[14] Kessler-Harris and Nancy Schrom Dye point to the effectiveness of the Women's Trade Union League in promoting the unionization of working women in the early twentieth century, but Kessler-Harris warns that class distinctions in the league coupled with Samuel Gompers's antipathy toward women's organizations delayed any possible drive for mass organization.[15] Since women were in mechanized industries, those who were not organized in the textile drives of the Amalgamated Clothing Workers of the Progressive era would wait for unionization until the successful CIO drive in the 1930s.

Susan Porter Benson concludes that saleswomen simply faced a stacked deck when it came to unionization: high turnover, the possibility for promotion, a paternalistic company union, and an effective anti-union policy by management. She argues that what is astounding is the number of attempts to create unions in the department store industry. Here Benson makes a point that cannot be overemphasized in the history of working women: women tried, often against overwhelming odds, to unionize. While Cooper's book chronicles a series of attempts by women cigar makers to unionize despite initial hostility from the CMIU, Milkman's book reveals how women, once unionized, were often isolated from male workers. Caught between pointing to their special status or entering unprotected into the fraternity, women trade unionists faced enormously difficult choices in finding a strategy that would keep working women unionized.

THE ISSUE OF LABOR UNION STRATEGY is addressed in three of the seventeen essays collected by Carol Groneman and Mary Beth Norton from the Sixth Berkshire Conference on the History of Women. Karen M. Mason approaches the labor union issue by examining the strategies of two women organizers, Josephine Casey and Pauline Newman, and assessing their impact in an unsuccessful Kalamazoo corset makers' strike in 1912.[16] Casey, a former ticket taker for the Chicago elevated railway and a union activist, was hired by the International Ladies Garment Workers Union (ILGWU) to organize the strike. She did so, using the Victorian notion of protecting women from exploitation. After Casey was released

[14] Barbara Wertheimer, *We Were There: The Story of Working Women in America* (New York, 1977); Alice Kessler-Harris, *Out to Work: A History of Wage-Earning Women in the United States* (New York, 1982).

[15] Kessler-Harris, *Out to Work*, 203–14; Nancy Schrom Dye, *As Equals and as Sisters: Feminism, the Labor Movement and the Women's Trade Union League of New York* (Columbia, 1980).

[16] Mason, "Feeling the Pinch: The Kalamazoo Corsetmakers' Strike of 1912," in Groneman and Norton, *"To Toil the Livelong Day,"* 141–60.

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from jail, Newman took up the leadership for her exhausted co-worker. Rejecting the tactic of female dependency, Newman relied instead on emphasizing the economic inequalities still remaining in the proposed settlement. Newman had no more success than Casey, but she was less reliant on middle-class women to rescue women workers. She hoped instead that a consumer boycott would bring the employers to their knees.

Mason argues that the different experiences of each woman shaped her expectations in the strike: Casey was trained in Chicago, where an activist female reform movement of the middle class had been successful in rescuing striking garment workers in the Hart and Schaffner strike through an impressive outpouring of support. Newman, on the other hand, had come through the Triangle Shirtwaist disaster in New York, first experiencing the defeat of the garment workers' strike, then a tragic fire that killed women locked in their work rooms, before middle-class reforming women had acted. Both women were tied to the Women's Trade Union League, although, as Alice Kessler-Harris has shown, the relationship between middle-class reformers and working-class organizers was far from ideal. In the end, the tactics of both women—moral suasion and consumer boycott—failed in the face of overwhelming corporate power. This experience in Kalamazoo seems not very distant from that of male workers on strike. Even the tactics of boycott and taking high moral ground were familiar to male strategists. Mason, however, points to what was fundamentally a female tactic, Casey's attempt to portray women workers as helplessly exploited, the proverbial damsels in distress. The consumer boycott was an appeal to female audiences. Newman called on women to assert some control through the power of consumption. Gender, then, shaped the tactics of women organizers. To American Federation of Labor (AFL) organizers, whose very concept of unionization was dependent on the ideal of manliness, the organization of women workers must have seemed an alien task.

While Mason illustrates the problem of style and tactics in women's organizing drives, Nancy Gabin contributes an essay revealing the difficulty of women unionists within the UAW during the same period as that discussed by Ruth Milkman.[17] Gabin finds that the Women's Bureau of the UAW was mere window dressing. She suggests that women failed in their attempts against discrimination during reconversion largely because male trade union leaders did not take women seriously. Again, it appears that cultural stereotypes helped to marginalize women at a crucial stage in trade union organizing. Gabin believes that gender-specific organizing in the UAW, while useful in giving women confidence, may have been a barrier to sexual equality in the union in the early 1950s.

Cynthia Costello argues nearly the opposite case in her study of women white-collar workers in the 1970s.[18] Feminist consciousness and support, Costello writes, was responsible for pressing office workers at the Wisconsin Education

[17] Nancy Gabin, "Wins and Losses: The UAW Women's Bureau after World War II, 1945–1950," in Groneman and Norton, *"To Toil the Livelong Day,"* 233–49.

[18] Cynthia Costello, "Working Women's Consciousness: Traditional or Oppositional," in Groneman and Norton, *"To Toil the Livelong Day,"* 284–302.

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Association Insurance Trust to unionize and strike successfully. To be sure, there were important differences in the situations of the two groups of union women. The United Auto Workers were in an industry that discouraged women from leaving the home, as Milkman tells us through Henry Ford. Women were induced to return to their homes, not by some nature-driven homing device, or by jealous union gangsters, but mostly by management itself. By contrast, the Wisconsin Education Association, though not officially a union, was acting as the official union representative of school teachers, giving new women unionists considerable leverage once they made their grievances known to other women unionists. Finally, the element of time is crucial in understanding a subtle shift in the reception of trade unionism among women. The feminist movement of the early 1970s made a point of destroying the basis for cultural stereotypes by ridiculing the myth made fashionable by Philip Wylie (but invented long before the first women cigar makers) that women are an idle, candy-eating lot, a generation of vipers. This ideological damage to old stereotypes, combined with the economic transformation that has placed women of all ages squarely in the paid labor force, presents a new stage for social change, and it challenges labor historians to rise to the occasion in their interpretations.

THIS SHORT REVIEW CAN ONLY EXAMINE A SMALL SEGMENT of the innovative research that is now being published on the history of working women. The seventeen articles in the Groneman and Norton collection aptly illustrate the variety and changes in women's work life over two centuries. The integration of gender—and, for that matter, race—into the historian's framework of analysis, in place of its previous utilization as a static category or a fixed association, presents new possibilities in the historiography of American labor. Cultural stereotypes about the role of women in the paid labor force have been persistently resisted by working women themselves, but only recently have historians begun to give substance to what feminists asserted fifteen years ago. The works of Milkman, Cooper, and Benson are representative of a wide range of new histories focused on the issues of gender and class. These studies indicate that working women created their own work culture, fashioning it from both the conditions on the work site and their own self-concepts, just as men did. These work cultures served to protect working people. They were not invented to preserve gender privilege, but they operated within an unequal society in which cultural stereotypes continually portrayed women as less aggressive, less competitive, and less interested in financial gain.

The recent Appeals Court decision in the case of *Equal Employment Opportunity Commission versus Sears, Roebuck and Company* illustrates the persistence of these cultural stereotypes. The case examined whether jobs carrying higher sales commissions were assigned preferentially to men over women. The lawyers for Sears argued that women were simply not interested in these more competitive positions. Calling on women historians who have written about the power of ideological persuasion in the inculcation of the culture of domesticity, the Sears

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lawyers argued that the company could not interest women in these jobs. In January, 1988, a Court of Appeals upheld the 1986 ruling in favor of Sears. Nevertheless, the dissenting opinion repeated what these histories of working women seem to tell: "There are abundant indications that women lack neither the desire to compete strenuously for financial gain nor the capacity to take risks."[19]

[19] *EEOC vs. Sears*, Appeals Court decision, January, 1988. My thanks to Alice Kessler-Harris for lending me a copy of the decision. See Kessler-Harris, "Equal Employment Opportunity Commission v. Sears, Roebuck and Co.: A Personal Account," *Radical History Review*, 35 (April 1986): 57–79.

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Reviews of Books

GENERAL

CHRISTA JUNGNICKEL and RUSSELL MCCORMMACH. *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein.* Volume 1, *The Torch of Mathematics, 1800–1870*; volume 2, *The Now Mighty Theoretical Physics, 1870–1925*. Chicago: University of Chicago Press. 1986. Pp. xxiii, 250; xv, 435. $120.00 the set.

The goal of this magnificent study by Christa Jungnickel and Russell McCormmach is to trace the stages by which original scientific research became institutionalized in universities, focusing on the rise of physics and ancillary mathematics. The locale is Germany during the formative years of the nineteenth and early twentieth centuries—including German-speaking cultural areas such as Austria and parts of Switzerland. A major thread of intellectual development is theoretical physics as we now conceive that subject.

In the early nineteenth century German theoretical physics came into being, drawing heavily on French mathematics. It was not until nearly midcentury that a body of German mathematical theory provided the base for further advances. The discovery of the fundamental law of electric currents, the familiar "Ohm's law," in the beginning of the century, illustrates this thesis. Georg Simon Ohm made his discovery by adapting to electricity mathematical principles used by the Frenchman Jean-Baptiste-Joseph Fourier in his elaboration of the theory of heat. It was Ohm who used the phrase "the torch of mathematics" to describe the intellectual mastery of nature.

Ohm's life struggles illustrate the problems in the career of physics in early nineteenth-century Germany. His work was too theoretical, too mathematical, to be easily appreciated by other physicists until a considerably later experimental verification. He was criticized by Hegelians and by those who were imbued with *Naturphilosophie*. He was refused a professorial post at Munich by colleagues who did not wish to share their limited apparatus with a possible rival (and who even denied its use to their assistants). By the 1850s, however, when Ohm finally attained a regular professorship, a physicist no longer was considered a mere writer of textbooks whose usual position was in a Gymnasium or technical higher school; Ohm had risen with his subject to become a researcher and full academic in an autonomous major subject in universities. This new position was accompanied by the granting of state funds for the support of research and the provision of time for professors to do research as part of their university function with full salary. Also important in the German scene was the creation of paid positions in local and national academies.

As this story is unfolded and analyzed, the authors show how the great figures who laid the foundations of what we now call classical physics also pioneered in the administrative revolution that created the modern research university. In a detailed, massive, fully documented presentation, the authors—drawing heavily on printed and manuscript primary sources—trace out and analyze both the development of physics topic by topic and the internal sociology, economics, and politics of science in the German system. Dealing with physicists as research scientists, as teachers, as members of a university administrative system, and as government employees, the authors have produced a magisterial contribution to social, educational, institutional, and intellectual history and a magnificent portrayal of the actual growth and content of the science with which they are concerned.

These volumes are replete with historical insights about science. For instance, the authors conclude that in the 1840s "established mathematical laws" rather than "new experimental findings" were, as often as not, "the starting point for the theoretical construction of further laws." Above all, the authors have demonstrated in analytical terms how "German physicists," from Ohm to Einstein, constructed, extended, criticized, and taught the "classical theories" and simultaneously how "they carried out this work within institutions they created or adapted for this purpose." Guided by what Hermann von Helmholtz called the furthering of "the intellectual

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mastery of nature," these men were less concerned with Helmholtz's parallel "material mastery of nature." These physicists were prime movers in establishing the currently accepted view of a university as a center where research and teaching are equal partners in the process of education.

I. BERNARD COHEN
Harvard University

JOHN G. BURKE. *Cosmic Debris: Meteorites in History*. Berkeley and Los Angeles: University of California Press. 1986. Pp. x, 445.

This is the first comprehensive account of the history of the scientific study of meteorites. After a brief introduction and a survey of eighteenth-century discussions, it concentrates on the technical development of meteorites, starting with Ernst F. F. Chladni's treatise of 1794 and explaining in some detail the application of astronomical, chemical, and mineralogical techniques in the nineteenth and early twentieth centuries. The book surveys theories of the origin of meteorites and concludes with the recent debate about the proposal by Luis Alvarez and his colleagues in 1980 that a giant meteorite or asteroid impact is associated with the Cretaceous-Tertiary transition sixty-five million years ago.

Since the mid-nineteenth century it has been suspected that meteorites have played a major role in the evolution of the solar system and may indeed be identical to the primordial material from which the earth, other planets, and their satellites were formed. Thus, meteorites have become the "poor man's space probe" in the words of a contemporary scientist (Edward Anders, quoted on page 148). In particular the Allende meteorite, which landed in Mexico in 1969, has furnished nearly as much information about the origin of the solar system as the multi-billion-dollar moon landings that started in the same year.

The current fashion in professional writings on the history of science is to emphasize the role of "external" factors such as social, political, and institutional influences on scientists and their research. According to this definition, John G. Burke's book is unfashionable, for it concentrates on the scientific aspects of the subject. Burke does not ignore external factors but introduces them only when they are clearly relevant to the science. For example, the recognition that meteorites do in fact have an extraterrestrial origin was impeded by widely held ideas about the heavens, going back to the period when Aristotelian philosophy was dominant. Yet, even here, as Burke points out, one must be somewhat skeptical about the social significance attributed to Thomas Jefferson's alleged remark that "it is easier to believe that two Yankee professors would lie than that stones would fall from heaven" (p. 57).

Burke has produced a remarkable work, characterized by careful scholarship and scientific accuracy, that will soon take its place on the shelf of authoritative reference works on the history of science. The casual reader who lacks a scientific background will find many chapters difficult to understand, but for those who have a serious interest in the subject this book will be indispensable. Its value is enhanced by an attractive large format, numerous illustrations, notes, a comprehensive bibliography, and three indexes (name, subject, and meteorite).

STEPHEN G. BRUSH
University of Maryland

RONALD L. NUMBERS, editor. *Medicine in the New World: New Spain, New France, and New England*. Knoxville: University of Tennessee Press. 1987. Pp. 175.

These three essays compare medicine in Spain, France, and Great Britain with their explants in Mexico, Quebec, and New England. Each focuses on the education and organization of professional healers and on hospitals, disease patterns, and medical theory and practices on correlative sides of the Atlantic. The discussions are thoughtful and inherently interesting, and editor Ronald L. Numbers has contributed a provocative introduction and an accurate summary. The sources cited are appropriately inclusive, and the book ends with an annotated bibliography that will be useful to historians interested in health care in any of the six geographic areas discussed.

According to Guenter B. Risse, the Spanish transplanted their characteristic medical bureaucracy—headed by officials called *protomedicato*—almost intact to the New World and built many hospitals as part of their pacification program. Medical schools were also established, but they failed to provide sufficient new practitioners. Spanish physicians, like their French and English counterparts, adapted surprisingly few indigenous plant remedies to the needs inherent in their traditional theories.

French doctors, too, carried medical concepts from the Old World to Quebec, but they organized themselves differently, without the traditional guild structure, if only because there was little competition among them. Toby Gelfand notes that military needs fostered the establishment of hospitals, but they were also patronized by the well-to-do. However, the province was less important to the French national economy than Mexico was to the Spanish, which probably tended to minimize governmental interference with colonial Canadian medicine.

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After the first immigrations to New England, most physicians were apprentice-trained in the colonies, not in universities as the French and Spanish required, although a few studied in London (Edinburgh was favored by Middle Atlantic physicians). There were no hospitals in colonial New England (and only three elsewhere in British America), although municipal charities did contract for physicians' services. Chiefly using data from Massachusetts, Eric H. Christianson has found that some professional organizations resembled guilds in their purposes, but, before the revolution, most were more concerned with self-improvement, control of quackery, or body snatching.

These are comprehensive studies by experienced observers of early Spanish, French, and English medical practices. Together they refute Frederick Jackson Turner's hypothesis about the influence of frontier conditions, at least on medical practices, as the editor explains in his provocative introduction. On the whole, colonial healers preserved the professional traditions of their homelands; the divergence of the New Englanders' medical organizations, and their lack of hospitals, can probably be ascribed to their background as Dissenters rather than as colonists who invented democratic responses to life in the wilderness.

J. WORTH ESTES
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K. DAVID PATTERSON. *Pandemic Influenza, 1700–1900: A Study in Historical Epidemiology.* Totowa, N.J.: Rowman and Littlefield. 1986. Pp. xvii, 118. $29.50.

Historical epidemiology is something of a stepchild of several different disciplines. Historians often study epidemics as part of social history. Epidemiologists with their base in medicine use biostatistics to help cure disease—not to understand the past. Medical anthropology and medical geography are subdisciplines that may become historical as they study change through time.

K. David Patterson's book is thoroughly historical, but it makes a contribution to the medicine of the epidemiologists who cure and to medical geography as well. It was, indeed, planned as a complementary volume to Gerald F. Pyle's *The Diffusion of Influenza: Patterns and Paradigms* (1986). Whereas Pyle is mainly concerned with the geography of influenza in the middle decades of the twentieth century, Patterson is concerned with origins of pandemic influenza on a world scale and especially with its appearance in the eighteenth and nineteenth centuries.

Neither author, curiously enough, gives a principal place to the great pandemic of 1918–19, which killed more people than all the military actions of World War I and more than any other single disease-appearance in recent centuries of world history. But that epidemic is already far better reported than those that preceded or followed it, and it was a unique event in known history. No other subtype of influenza behaved in quite the same way, killed so many people, or killed people at such young ages. Both Patterson and Pyle are therefore justified in looking more narrowly at the varieties of influenza whose comparative study yields significant results.

Patterson's contribution is partly a detailed description of the origins and movement of the principal pandemics, especially those of 1830–31, 1833, and 1889–90. He worked from a variety of sources, especially from secondary accounts of influenza in particular regions, which provided detailed reporting for most Western countries in that period. He draws this material together, however, with some supplementary help from sero-archaeology, for a world-wide view capable of reaching broader conclusions.

He shows influenza pandemics to have mainly originated in central Asia from a single source of genetic drift, which shifted the nature of the virus enough to avoid the immune responses of most victims. The pandemic could then move outward at the current speed of transportation, attacking from one-third to one-half of the population, but with very low case-mortality rates. Large numbers died nevertheless, as many as one per thousand in the population at large, but with age-specific mortality heaviest among older people. The major influenza pandemics in nineteenth-century Europe killed even more people than the more spectacular cholera epidemics of the 1830s or 1850s. Patterson thus helps redress the balance by calling attention to a disease whose social consequences have been comparatively neglected.

PHILIP D. CURTIN
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MARTINE SEGALEN. *Historical Anthropology of the Family.* Translated by J. C. WHITEHOUSE and SARAH MATTHEWS. (Themes in the Social Sciences.) New York: Cambridge University Press. 1986. Pp. x, 328. Cloth $49.50, paper $14.95.

Martine Segalen's book represents an attempt to review and synthesize the wealth of literature on the family that has emerged over the past two decades in various disciplines. Although the title probably was chosen because concepts such as descent group, kindred, and alliance figure prominently and because the approach is generally cross-cultural, it is also somewhat misleading. One of the great strengths of the book is to introduce a range of

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European literature on the sociology of the contemporary family that is little known and rarely cited by North American scholars of family history.

Segalen sets as her overall goal an exploration of "how the family has lived through the economic, social and cultural upheavals of the last hundred and fifty years, how it has resisted them and how it has contributed to them" (pp. 2–3). The book is divided into three parts. Although much of the first part—a review of basic concepts—hardly differs from discussions in anthropology texts on kinship, it is clearly written, and historians may find the juxtaposition of European material with that deriving from more traditional areas of anthropological research illuminating. The author begins by outlining several types of large domestic groups—the tacit community, the zadruga, and the stem family—that "appeared, functioned and finally disappeared in particular social and economic contexts" (p. 20). This leads to a more general discussion of the varying size and structure of domestic groups and how they change through the life cycle. The analysis of filiation draws on a range of cross-cultural data from the Trobriand Islands to the Shoshone Indians. It is rooted in a Levi-Straussian framework. The other major issue taken up in part 1 is the relationship between industrialization, urbanization, and changes in the family. It includes an interesting treatment of the role of kinship networks as "mediate institutions between the individual and society" (p. 98).

Part 2 focuses on the formation of domestic groups from marriage through the birth of children. Religious, demographic, and cultural constraints on marriage are reviewed. The author surveys the relative importance of contracts of love as opposed to those of interest in a variety of cultures. By integrating contemporary data, she is also able to write about divorce and conclude that it is, paradoxically, a "sign of the health of the institution of the family" (p. 157). The chapter on children traces the development toward a two-child norm and the changing relationships between parents and children over time and across social classes. Segalen notes a dearth of research on parent-child relationships at various stages of the developmental cycle and in addition encourages more research on sibling relationships.

The third part of the book treats domestic roles. The author begins by raising the problem of dealing with this question historically. Conversely, sociological and anthropological literature provide a wealth of material on the division of domestic tasks, family budgets, the differing statuses of men and women, and the impact of women's employment on domestic roles. Chapter 8 contains a succinct discussion of various sociological role theories. Some intriguing illustrations of floor plans lead the reader to speculate about the changing impact of domestic space on domestic roles. A final chapter on the family and society addresses the issue of social control: the laws and rules that constrain the freedom of the family, as well as the power that the family itself has over individuals.

Though at times somewhat dry, Segalen's volume is thorough. Each chapter concludes with a list of suggested readings, a useful addition for those who are beginners in the field of family studies. In general the book appears to achieve the purpose of the series in which it is published—to develop books of general and interdisciplinary interest that can be used by teachers, researchers, and undergraduates. Old-hand scholars of the family will turn to it more as a quick source for bibliography than for new theoretical insights. The book lacks a conclusion—a synthesis of the author's synthesis.

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JACK GOODY. *The Logic of Writing and the Organization of Society*. (Studies in Literacy, Family, Culture, and the State.) New York: Cambridge University Press. 1986. Pp. xvii, 213. Cloth $34.50, paper $8.95.

Many historians will associate the name of Jack Goody with "The Consequences of Literacy," written with Ian P. Watt and first published in 1963 in *Comparative Studies in Society and History*. Goody and Watt used the example of ancient Greece to point to the wide range of institutional and cognitive effects that follow closely from the societal acquisition of alphabetic literacy, most crucially, in their view, bureaucracy and a cumulative capacity for criticism of traditional thinking. The case now seems to be sweeping and deliberately provocative, but it has exerted a wide influence through the impetus it gave to a comparative and developmental approach to research on literacy across history, anthropology, sociology, and applied linguistics.

In the present book Goody meets those of his critics who have accused him of "communicative determinism" by addressing in detail the question of how the transition of a cultural setting from a near-total dependence on oral communication to an extensive institutionalization of writing interacts with features of the "surrounding" social structure. The principal spheres dealt with are religion, economic activity, the state and political administration, and the law. The discussion of the preliterate position is based on material relating to contemporary West African societies, whereas the protoliterate situation is exemplified by the ancient Near East.

Goody contrasts alphabetic religions, which spread through conversion, with oral sects, shrines,

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and movements, which remain territorially bound and tied to a local way of life. Religions codified in sacred texts offer a canonical core of belief more or less resistant to change and universalistic norms to be applied to conduct, whereas orally transmitted ones are flexible and constantly making fine adjustments to the shifting local situations. The religions of the book tend to spawn priestly specialists, accumulate property, and develop toward a bureaucratic autonomy, whereas orally based myths and religions tend to be homeostatic, "a kind of ever-adapting charter for social action." The writing down of legal judgments and codes and the emergence of legal specialists give rise to a parallel process of institutional differentiation.

The role of writing in the overlapping activities of palace and temple and the necessity for quantitative records, accounts, and tokens of value to accompany innovation in trade, credit, land transfer, and taxation are dealt with in the second chapter. It is suggested that the widespread use in Africa of a "segregated" medium of exchange (cowrie shells, salt bars, or cloth bolts) has placed limits on the complexity of credit and other commercial arrangements. Goody further sees the absence of an indigenous written tradition (outside the Islamic sphere) in the West African savannah states as contributing to their inability to resist European cultural and political colonization.

For its selected regions, this book presents some of the detailed evidence and argument lacking in Goody's schematic original essay. This material is organized in a skillful and efficient manner, but more is apparently to come (*The Interface between the Written and the Oral*), and it would be premature to judge either the precise form or the strength of Goody's final conclusions about literacy.

KENNETH LEVINE
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ABEL POITRINEAU. *Les mythologies révolutionnaires: L'utopie et la mort.* (Histoires.) Paris: Presses Universitaires de France. 1987. Pp. 258. 140 fr.

At the conclusion of this book, Abel Poitrineau rhetorically asks how to weigh the good that revolutions do against the suffering they cause. This is as close as the book gets to hesitation. For the most part, revolutions are condemned as the sad inspirations of intellectuals in love with their own intolerant fantasies, bringing bloodshed in their wake while stringing along mobs as crazed as Le Bon ever imagined.

Taine rides again, but with more pretension and less grace. The book is divided into two sections. In the first stirring ideas are dissected, starting with definitional distinctions between utopias, myths, and ideologies (is millenarianism myth or Utopia? probably the latter), drawing mainly on early modern Western examples. From their initial generation, ideologies and their mates follow a standard schema. They are transformed—systematized—into revolutionary plans. They spread in the epidemic phase; this section dwells a bit on means of transmission but says little about actual social reception, for the author earlier concluded that sweeping concepts gather followers "like streams flow to the river" (p. 20), meaning that much detail is unnecessary. Then comes a violent breakout, tending to lead toward despotism as disorganization pushes the revolution further, and certainly leading to blood and terror. The pattern suggested will hardly surprise those who have encountered revolutionary anatomies elsewhere.

The book's second, even more lightly footnoted, section briefly details a number of revolutionary episodes: the Hussites, the Anabaptists, the English Civil War (in a chapter entitled the glorious revolution but ending in 1660), France through the Terror, Haiti, Russia from 1917 through the mid-1930s, and, finally, Cambodia. There is no rationale offered for this selection rather than another. There is no explicit comparison. Some chapters deal with ideology as well as offering a brief (and, to be fair, aside from inconsistent terminology, usually competent) narrative, but others just have the narrative (notably those on Haiti and Cambodia, where the ideological dynamic largely fades from sight). All, of course, concentrate on terror, usually ending on this theme.

Save perhaps as an illustration of aspects of the French political mood of today, the book is of little use. Its summary strengths are easily surpassed in other available treatments. The author punches up his argument with hosts of dynamic images, referring to revolutionary thermodynamics and explosions. Yet the net effect is less bang than whimper.

PETER N. STEARNS
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LUCIANO PELLICANI. *Miseria del marxismo: Da Marx al Gulag.* (Argomenti, number 103.) Milan: Sugar. 1984. Pp. 366. L. 25,000.

Luciano Pellicani's study of Marxism can be approached on two levels. It is an unrelenting attack on Marx and on his legacy to socialism. From this point of view, however, the book offers little that is new or original. Pellicani's work can also be read as a manifestation of the current political mood within Bettino Craxi's Italian Socialist party (PSI). Pellicani himself occupies a dual role as an Italian academic and as director of the socialist review, *Mondoperaio*, and member of the PSI political leadership. His

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book becomes a semiofficial statement of a democratic, anti-Marxist ideology in a party that until 1956 was noted for its adherence to revolutionary Marxist traditions. It takes on even greater meaning in light of the current maneuvering between the PSI and the Italian Communist party for hegemony on the Left. It is a clear attempt to exclude the Communist party from the democratic, pluralistic values that inspired Western social democracy.

Pellicani considers Marxism to be a secular religion with all the drawbacks of a fanatical cult. At its core, Marxism was a myth that was defined in negative terms as anticapitalism not in positive terms as a well-defined and realizable set of goals. Marx hid the emptiness of his vision by making it part of an inevitable historical process that he arbitrarily called scientific. Pellicani borrows from popular psychohistory for his interpretation of Marx as inspired by the desire to become God. Driven by a messianic thirst for absolutes, Marx separated the world into incompatible spheres of good and evil or salvation and damnation. The result was a philosophically totalitarian vision that inevitably translated itself into political terms. Marx's own polemical debating style is seen not just as a manifestation of an irascible personality but as a true psychological disorder.

Pellicani sees Marx's rejection of liberal society as basic to his philosophy and quite independent of capitalist economic exploitation. Marx was viscerally hostile to the pluralistic outlook of an open society. The alienation that Marx understood as part of human existence could only be overcome by moving into a radically different society. This new society claimed a monopoly over the future, but it was in fact a return to the religious and mythic systems of the past.

Marxism was never scientific or empirical. The outcome of the historical process had already been predetermined, and the facts organized to fit the philosophical premises. As an antidote to Marx, Pellicani advocates a return to Proudhon for a federalist, individualist, and anticollectivist socialism and to Bernstein for neo-Kantian, empirical, humanistic revision of Marx. Pellicani concludes that a total incompatibility exists between the political movements inspired by Marx and democratic socialism. He makes no distinction between the actual totalitarianism of Stalin and the philosophical totalitarianism of Marx. Here again, the break with the past of the PSI, which had been dominated by endless debates over united fronts with the Communists, is remarkable.

As intellectual history, there is nothing new here. Moreover, the book is made up of a series of articles, written for many diverse occasions but not edited to eliminate superfluous and repetitive material.

ALEXANDER DE GRAND
North Carolina State University

EDOARDO TORTAROLO. *Illuminismo e rivoluzioni: Biografia politica di Filippo Mazzei.* Milan: Franco Angeli. 1986. Pp. 221.

Tuscan by birth and American by adoption, Filippo, or Philip, Mazzei (1730–1816) played an important role on both sides of the Atlantic from 1773 to 1792. At home in London, Virginia, Paris, Warsaw, and his native Tuscany, he participated in the major happenings of his time. His friends and patrons included Franklin and Jefferson, Condorcet and Lafayette, Grand Duke Leopold of Tuscany and Stanislas Augustus Poniatowski, the last elected king of Poland.

Edoardo Tortarolo, a young Italian historian and disciple of Franco Venturi, analyzes Mazzei's role as a conduit of ideas between the American colonies and Europe, particularly France and Poland. For Tortarolo, Mazzei's importance transcends his role in the American revolution, emphasized in earlier studies on Mazzei by Margherita Marchione and Howard R. Marraro.

Mazzei's *Recherches historiques et politiques sur les Etats-Unis de l'Amérique septentrionale, où l'on traite des établissemens des treize colonies, de leurs rapports et de leurs dissensions avec la Grande-Bretagne, de leurs gouvernemens avant et après la révolution* was published in Paris in 1788. Written with Jefferson's support and encouragement to rebut the criticisms of Mably and Raynal, it offered a balanced defense of the American revolution and seems to have been widely read, as Tortarolo's detailed analysis of its impact brings out, despite low sales.

In the two chapters summarizing the extensive debates on the relevance of the American experience for Europe, Tortarolo examines the heated polemics in France, where political life became increasingly polarized between moderate reformers and Jacobins after 1789. To the discussions in Condorcet's *Société de 1789* and France through the early 1790s, Mazzei, a trusted member of Condorcet's circle, contributed a firsthand appreciation of the American revolution. Although passionately devoted to its ideals and principles, Mazzei nonetheless believed that reform in France would be better effected under an enlightened monarch than under a republic on the American model, for which Europeans were not prepared.

Tortarolo gives short shrift to the last twenty-four years of Mazzei's life, succinctly covered in little over ten pages. Since Tortarolo's interest was in Mazzei's involvement in the two major revolutions of the century, it might have been better to have concluded with the end of Mazzei's active participation in politics. This is a minor cavil to a brilliantly written and scrupulously documented work that makes a

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real contribution to our understanding of the circulation of ideas in the Atlantic community.

EMILIANA P. NOETHER
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GREGORY U. RIGSBY. *Alexander Crummell: Pioneer in Nineteenth-Century Pan-African Thought.* (Contributions in Afro-American and African Studies, number 101.) New York: Greenwood. 1987. Pp. xviii, 231. $32.95.

Gregory U. Rigsby's study focuses on Alexander Crummell's pan-African ideas and his spiritual approach to life. For Crummell pan-Africanism, or the unification of all peoples of African descent, was but one step in his goal of human unity. Crummell approached life with an awareness of a transcendental order in the world.

Crummell was born free in New York City on March 3, 1819. The earliest influences on Crummell's thinking were his father, Boston Crummell, from whom he adopted the ruling thoughts of his life—Africa, race, and aristocracy—and Peter Williams, Jr., who helped develop Crummell's pan-African consciousness.

Up to 1848, the year he left America for England, Crummell displayed a strong pan-African identity. Whenever he mentioned the manifest destiny of black people, he always included all black people, those in Africa and those abroad. Crummell believed that black people had to realize that it was their duty, not optional right, to commit their lives to the welfare of their people.

Despite prejudice, hostility, and insults, Crummell was not easily daunted in his pursuit of education. He attended Williams's school, African Free School No. 2 (both in New York City), Noyes Academy in Canaan, New Hampshire, and Oneida Institute (Whitesboro, New York) and then decided to become an Episcopal priest. Crummell was rejected by the General Theological Seminary (New York City) and apparently attended an institution in Camden, New Jersey. In December 1844 he was ordained to the priesthood by Alfred Lee in Philadelphia. But Crummell continued to have difficult financial times in his church work in America and decided to go to England at the beginning of 1848 to raise funds to build a black church in New York.

Crummell remained in England for five years. While in England he attended Queen's College, Cambridge University, earning the B.A. degree in 1853. In England Crummell's international pan-African dream began to take definite shape, and he became an internationalist in his pan-African concerns. Crummell's pan-African position rested on three premises: the idea of racial solidarity among black people wherever they might be, the belief that each people had characteristics peculiarly suited to itself, and the rightness of Christian "civilization" to guide black people to a glorious future.

In 1853 the Foreign Mission of the Domestic and Foreign Missionary Society in America offered Crummell a salaried appointment as a missionary in Liberia, and he eagerly accepted the job. Crummell remained in Liberia almost twenty years. Crummell had been imbued with and had accepted the values of Christian Western "civilization," and these values he proposed as the goal to be achieved by Africans.

While in Liberia Crummell became obsessed with the idea of building a church in Monrovia. But the project for erecting a church building, which eventually centered on Crummell's inability to oversee financial matters, and the mulatto-black conflict proved to be the final undoing of Crummell in Liberia. He arrived in Liberia with enthusiasm and optimism, but he left in April 1872 physically sick and spiritually debilitated. The optimistic and determined missionary had explored fully the notion of an international pan-Africanism, but the idea proved to be premature.

On his return to America, Crummell took charge of St. Mary's Chapel, later St. Luke's Episcopal Church, in Washington, D.C., where he remained until 1894. In the last years of his life Crummell helped found the American Negro Academy, and he remained president for eighteen months until his death on September 10, 1898.

The author argues that racial solidarity thinking passed from "Cuffe to Williams to Crummell to DuBois and Garvey, to Malcolm X" (p. 6) and compares Crummell's concern for the condition of black people throughout the world with Marcus Garvey's appeal to "Africans at home and abroad." This viewpoint seems to perpetuate the erroneous interpretation that pan-Africanism has been only an idea shared by a few black leaders at different historical periods, instead of a continuous thread in the Afro-American experience in America. Throughout the book the author attempts to tie Crummell to Garvey. It is obvious that the author greatly admires Crummell, and this may explain his occasional exaggeration of examples of Crummell's pan-African thought.

Additionally, the book is sometimes confusing and difficult to follow. In many places the discussion is organized topically, rather than in chronological order, and the author skips back and forth in his discussion with no continuity. My final criticism is that it is often difficult to distinguish between the ethnocentrism and sexism of Crummell and Rigsby. Although this chauvinism is understandable coming from the nineteenth-century Crummell, it is inexcusable in the author.

Notwithstanding these shortcomings, Rigsby has added to the literature on nineteenth-century pan-

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African thought and contributed to our knowledge of this important period in the development of pan-Africanism.

SYLVIA M. JACOBS
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MARIANNE KRÜLL. *Freud and His Father*. Foreword by HELM STIERLIN. Translated by ARNOLD J. POMERANS. New York: W. W. Norton. 1986. Pp. xvii, 294. $18.95.

In his preface to Marianne Krüll's book, Helm Stierlin, whose work in family therapy Krüll credits with inspiring her approach to Freud, writes, "No doubt the author's psychohistorical method has certain limitations. Time and again gaps appear . . . several explanations suggest themselves for . . . the same fact . . . she repeatedly stresses the conjectural character of many of her interpretations and assumptions" (p. xiii). Stierlin's caution is well taken, but the book has even more serious problems than he suggests.

Krüll starts from the position that Freud's original "seduction theory" (actually, child sexual abuse theory) of neurosis is basically right and that his later theory of infantile sexuality is wrong. Therefore, the reasons Freud gave for abandoning the seduction theory could not have been his real reasons. What follows is a reconstruction of those "real reasons" that not only is wildly speculative but also, because of its theoretical preconceptions, rejects certain kinds of available evidence a priori.

According to Krüll, all Freud had to do to develop a "true psychoanalytic theory" was to rid seduction theory of its "extreme fixation" on sexual seduction and expand it into a notion that parents often deceive a child about family happenings, thus promoting neurotic behavior (pp. 69–70). Freud could not take this step, however, because of an unconscious "mandate" given him by his father, Jacob, not to delve too deeply into his past. The search would have yielded guilty secrets about Jacob's sexual misconduct and his betrayal of his faith. Surrendering to this mandate, Freud gave up the seduction theory in favor of the Oedipal theory, which enabled him to take on himself the responsibility for his father's guilt. His own Oedipal fantasies were not the result of a universal Oedipus complex; they derived from an actual affair between his mother and his half-brother Philipp without which it would not have occurred to Sigmund to try to possess her (p. 128).

That Freud's self-analysis was both incompletely reported and incomplete and that real events played a role in shaping Freud's conflicts—for example, his devaluation of his father—are accepted by even the most "classical" analysts. Most reconstructions that go beyond Freud's self-disclosures, however, are based on established biographical facts or on Freud's recollections and associations. The researches of Joseph Sajner, Peter J. Swales, and Renée Gicklhorn, on which Krüll bases her interpretations, have revealed important new information, but they do not contain a shred of evidence for Jacob's pecadillos or his guilt or for an affair between Amalie and her stepson. The putative "facts" are not even useful as hypotheses. The events they purport to explain are either nonexistent, demanded only by Krüll's theory, or, if real, more plausibly explained in less tortuous ways.

Krüll's dismissal not just of the truth but of the rationality of Freud's thinking leads her to underestimate or ignore the role Freud's theoretical assumptions played in helping him reach his new conclusions. She cites, for example, the important letter of May 2, 1897, in which Freud suggests that not simply memories but "impulses drawn from the primal scenes" are repressed in hysteria. This letter, however, was written before Freud began his self-analysis. Krüll is unable to appreciate how the theory of "hallucinatory wish-fulfillment" based on prior "experiences of satisfaction," first outlined in *Project for a Scientific Psychology*, led Freud to interpret childhood sexual fantasies as evidence of childhood sexual impulses. The leap from a judgment of false theory to one of irrational theorizing leads to a mistaken reliance on psychological explanation to the exclusion of evidentiary and theoretical factors in the logic of discovery.

In Krüll's book the history of Freud's thought has become a casualty in a contemporary battle over the validity of his theory. Jeffrey M. Masson's *The Assault on Truth* (1984), which offers a rival psychological explanation for Freud's abandoning seduction theory, is another in what has become a minor genre. Krüll sharply repudiates Masson's explanation and method, but her own approach is similar in its instrumental use of psychobiography to discredit one theory and establish another. Classical theory may be right or wrong—various trends within clinical (including classical) psychoanalysis also argue for the clinical significance of real traumas and deficits in parenting. The relevant evidence in the dispute, however, is clinical and observational. Krüll's history is poor history not because it is psychohistory but because it has a theoretical axe to grind.

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PHILLIS GROSSKURTH. *Melanie Klein: Her World and Her Work*. New York: Alfred A. Knopf. 1986. Pp. x, 515. $25.00.

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Phyllis Grosskurth's biography takes on a formidable task. Melanie Klein's "world" includes her birthplace, Vienna in 1882, her younger years in Budapest, her psychoanalytic apprenticeship in Berlin in the 1920s, and her extraordinary career in London from 1926 to her death in 1960. Her "work" includes strikingly original and often bizarre contributions to the psychoanalytic theory of early childhood development, psychosis, and female sexuality and to the technique of child analysis. Grosskurth has gathered much interesting material about Klein's family and about how her own experience as a daughter, sister, and mother affected her thinking once she entered the world of psychoanalysis after reading Freud's *Interpretation of Dreams* in 1914. Not long after the end of World War I she presented her first psychoanalytic paper and became a member of the Budapest and then the Berlin Psychoanalytic Societies.

Klein's pathbreaking work was based on her analysis of children, and Grosskurth shows how her own children were used to investigate childhood anxiety and the proper psychoanalytic reaction to it. It is obvious that her children suffered much from their mother's psychoanalytic practice, and the ways in which her colleagues later passed children and grandchildren among one another for analyses gives new meaning to the idea that psychoanalysis attaches great importance to incestuous relationships.

The biography contains fascinating material on just how abusive psychoanalysis can be. Klein, who wrote much on the importance of transference and countertransference, appears to have been blind to her own use of analysis as a technique for controlling people with less power than she. Although Grosskurth reveals much that is shocking in this regard, she thinks that Klein learned from her mistakes and that others benefited from the knowledge she gained. Given the evidence presented in this biography, however, it seems that Klein repeated with her students and colleagues some of the tactics she used with her children. How Grosskurth, or anyone else, could know whether Klein's influence on others was beneficial remains completely unclear.

Klein's psychoanalytic inquiry concentrated on the earliest stages of infant development. She argued for the importance of the mother-child relationship in the dynamic of the infant's psyche. Furthermore, she took more seriously than most analysts the centrality of aggression for very young children and brought out the importance of anxiety, guilt, and depression for them. In other words, she pushed the focus of psychoanalysis back from the Oedipus complex to the first objects in the infant's world. The object-relations school of psychoanalysis in Britain and elsewhere has continued to cultivate the ground that she first cleared.

Much of the biography describes the ways in which Klein's ideas and techniques were either embraced or resisted by other analysts. Klein's great competitor in the field of the analysis of children was Anna Freud. Whereas Klein thought Freud's daughter superficial, Anna Freud thought Klein went too far too quickly in provoking, not discovering, childhood anxiety. Whereas Anna Freud strove to build up an ego that would adapt to the world around it, Klein plunged into what she thought was the child's unconscious so as to uncover what was blocking development. Grosskurth is clearly partial to Klein, but for reasons that remain obscure. Both women used methods to gain influence that had little to do with any intellectual claims but that had much to do with the power and money that came from controlling patient referrals. The reader sees why Klein thought Anna Freud superficial, but the biographer would have served her subject better by taking Freud's criticisms of Klein more seriously than she does.

One facet of the history of psychoanalysis emerges very clearly in Grosskurth's account. Women played an extraordinary role in the development of psychoanalysis in England, both in the development of theory and in the expansion of clinical practice. Was England different from other countries in this regard? Sigmund Freud often worried that psychoanalysis might be ignored as merely a Jewish science; one wonders how the sexism of Klein's world affected the reception of her ideas in it.

In reading this very interesting and informative book, an outsider to psychoanalysis will probably wonder why reasonable people are arguing about such strange things as a normal infant's schizoid or depressive position or the ways in which an infant's hostile feelings toward the breast are turned against his or her self. A reader who comes to the book with a sense that these are things worth talking about will probably want a more detailed theoretical consideration than this book provides of the importance and the difficulties that these ideas create for psychoanalytic thinking. However, Grosskurth has made an important contribution to our understanding of the history of psychoanalysis, the connection of that history to the personalities of its leaders, and the development of their ideas. For anyone concerned with psychoanalysis after Freud, this book is essential reading.

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RICHARD NED LEBOW. *Nuclear Crisis Management: A Dangerous Illusion.* (Cornell Studies in Security Af-

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fairs.) Ithaca: Cornell University Press. 1987. Pp. 226. $24.95.

This volume draws on a handful of twentieth-century international crises, especially that of July 1914 preceding the outbreak of World War I, to examine the management of nuclear crises. At the present time, Richard Ned Lebow writes, crisis management in the United States, with its emphasis on "organizational structures and decision-making techniques," bears "a disturbing resemblance to the ancient art of alchemy" (p. 18). Rather than searching for new techniques and technologies to avert or defuse crises, we need to focus more closely on the political and psychological dimensions of crisis management. This is the avowed purpose of Lebow's brief book.

The next major war, the author concludes, is likely to result neither from a "bolt-from-the-blue" strike nor from an accident, an act of terrorism, or the machinations of a third party. If there is another global war, it will most probably come about either as a miscalculation or as an act of desperation in a crisis or conventional war. "The paradigm of such a situation," Lebow warns, "is 1914," and the origins of World War I hold significant lessons for today's nuclear strategists. More alarmingly, Lebow finds "many disturbing parallels" between 1914 and the present (pp. 24, 104).

The central military lesson of the July 1914 crisis, Lebow states, is that strategy must be subordinate to policy. The Schlieffen Plan, by contrast, made politics subservient to strategy in order to capitalize on Germany's military advantages. In looking back to 1914 for contemporary relevance, this study focuses on three causal sequences to war: preemption, loss of control, and miscalculated escalation. All three, Lebow writes, were instrumental in bringing about war in 1914. Each of these three could act in a similar fashion today.

The historian is bound to ask whether the author uses his historical case studies judiciously. By and large the answer is in the affirmative, although one must qualify this by adding that Lebow's treatment of each crisis is selective rather than comprehensive. Nowhere does he set forth an extended analysis of the three crises to which he gives most attention—1914, the missile crisis of 1962, and the American strategic alert in October 1973 during the Yom Kippur War. This is intended not as criticism but merely to warn those who might otherwise expect a thorough discussion of these three episodes.

Lebow concludes on a mildly upbeat note. Our expectations about the inevitability of war make war less likely today than it was in 1914. Moreover, there are practical steps that can be taken to reduce crisis instability and thus the likelihood of war. Lebow's discussion of some of these steps will interest national security analysts far more than historians. But his underlying purpose—to make a nuclear Sarajevo less likely—must of necessity compel the attention of us all.

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ANCIENT

ISRAEL EPH'AL. *The Ancient Arabs: Nomads on the Borders of the Fertile Crescent, 9th–5th Centuries B.C.* Jerusalem: Magnes or E. J. Brill, Leiden. 1982. Pp. 265. $32.34.

Based on a solid philological foundation, Israel Eph'al's work on the ancient "Arabs" presents an exhaustive discussion of written sources, a systematic chronicle of known events for the period of the Assyrian empire, and a few forays into matters properly historical. Let me point out at the onset the nature and limits of these historical sections, since they are the ones of special interest to readers of the *AHR*.

An interesting section (pp. 93–100) deals with the issue of the integration of nomads into the Assyrian administrative system during the latter part of the eighth century B.C. The point is made here that nomadic groups were occasionally given preferential treatment over other population groups (see chapters 5 and 6 for the later periods) in order to lessen the need for a direct deployment of Assyrian forces along the edge of the steppe. On pages 126–27 and 137–38 there are some good observations on the role that the policy of relative appeasement toward the nomads played in Assyrian tactics when dealing with both Babylonia and Egypt. Also insightful is the remark (pp. 106–08) that the "deportation" of Arabs to Samaria was in fact quite advantageous to the Arabs as well as the Assyrians when viewed in the light of economic policies that were unique with Sargon ("The diversion of some Arab trade . . . increased Assyrian control and augmented imperial profits" [p. 107]).

Apart from these and a few other insights of genuine historical import, the work primarily remains strong for its philological merits. The data are presented clearly and accurately (although one misses, for example, a bibliography), and the information contained provides an excellent scaffolding for further historical analysis. That such a historical perspective is in fact an unrealized potential can be noted, for instance, in the lack of sufficient definition of the historical entity under consideration. The title presents it as, presumably, an ethnic group (Arabs), and the subtitle qualifies it in terms of life

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style or cultural adaptation (nomads). But a definition of the two concepts is given only very summarily in a brief introductory section (pp. 5–11), which develops rapidly into a philological discussion of the use of the term "Arab" in the cuneiform sources. And, although the data from cuneiform sources primarily include references containing these terms, the section on biblical sources is much more diffused (and is then not taken up in the section entitled "Historical Survey"). There is, in other words, a certain unevenness in the selection of sources that derives from a rather uncritical definition of the basic subject of inquiry.

Eph'al pays close attention to the cuneiform sources wherever the term "Arab" occurs to the exclusion of other dimensions that would have been appropriate for a fuller historical consideration of the problem. For instance, the evidence of archaeology and the argumentation from linguistic evidence are dismissed too quickly and are then at times introduced only indirectly into the discussion. For example, on page 9 it is stated that "the term 'Arab(s)' in this book has no linguistic meaning," but then on page 115 we read that "the designation 'Arabs' in this case is based on onomastic and linguistic criteria as well as on way of life." One also misses a sense of perspective vis-à-vis later developments, as known from either the historical or the ethnographic record, as well as certain contemporary theories that are pertinent even if one might disagree with them (I am thinking, among others, of Van Seters's reinterpretation of the Patriarchal tradition—a reference to whose work would seem particularly appropriate, for instance, on page 177).

One may object that I am seeking what the author does not purport to offer. I readily grant that this objection is correct to the extent that the author deals with "history" as generally understood within the field of ancient Near Eastern studies—the chronicling of events sorted from the written sources. But I would stand by my conclusion if by history we mean a more structural understanding of institutions and of process—of which the author gives only a few, however tantalizing, glimpses.

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EVA CANTARELLA. *Pandora's Daughters: The Role and Status of Women in Greek and Roman Antiquity*. Translated by MAUREEN B. FANT. Foreword by MARY R. LEFKOWITZ. Baltimore: Johns Hopkins University Press. 1987. Pp. xiv, 229. Cloth $30.00, paper $9.95.

L'ambiguo malanno, which was published in 1981, has been translated into English as *Pandora's Daughters*. Literally translated, the title means "the ambiguous evil." Eva Cantarella attempts to prove that from the prehistoric to the Byzantine period women were consistently regarded as malevolent and inferior and were "canceled from history." The author marshals evidence selectively in order to draw attention to two exceptions in what she considers thoroughly misogynistic millennia: the cult of Isis and the teachings of Christ.

Cantarella announces her intention to give priority to legal texts in reconstructing the lives of women. For the Greek world, however, most of the legal evidence is not codified nor of sufficient quantity to serve as the principal evidence for women's history. Thus, the author begins her discussion of Greek women with the Homeric poems, which she claims describe women in the Bronze Age (p. 32). The Dark Age is skipped completely as the poetry of Hesiod is ascribed to "the period immediately following" (p. 33). Then we are told that "the so-called dark ages were characterized by a certain flexibility that allowed women some freedom of movement" (p. 39). No evidence is cited in support of the last statement; in fact, there is none. There are no chronological or genealogical charts that might help clarify the confusing expositions typical of this book.

Cantarella is on surer footing in the Roman world, but her almost exclusive use of legal evidence distorts the historical picture. She dates the "emancipation" of Roman women to the period between the principate and the empire (p. 140), because at that time women's legal disabilities began to be lifted. However, historical texts including the works of Polybius, Livy, and Plutarch reveal that two centuries earlier, after the second Punic War, upper-class women began to act independently and to manage large fortunes, exploiting legal dodges and amenable guardians. Although the author states that this book is intended for a nonspecialist audience, the lack of subtlety sometimes leads to outright error. Thus, Cantarella's assertion that marriage "gave a woman a new master" (p. 117) ignores the possibility of *sine manu* marriage (which is mentioned as early as the XII Tables) and fails to take into account the consensual basis of Roman marriage. Her discussion of *partria potestas* glosses over the reality that this institution could be more oppressive to adult men whose sisters had left the paternal household on marriage.

Although Cantarella is a professor of Roman law whose analyses of legal language are often original and illuminating, this English version of her book can be recommended for neither students nor scholars. Citations of primary sources are frequently inadequate, and modern secondary works in English have been inserted in the notes without warn-

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ing the reader that these are not the works on which the author bases her arguments.

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ERALDA NOE. *Storiografia imperiale pretacitiana: Linee di svolgimento.* (Pubblicazioni della Facoltà di Lettere e Filosofia dell'Università di Pavia, number 34.) Florence: Nuova Italia. 1984. Pp. 156. L. 19,750.

This slender, learned, well-written volume considers Tacitus's predecessors, from the period of the civil wars at the end of the Republic. The purpose of this inquiry, in a field that has been well plowed over the years, is to determine how original, how rebellious a historian Tacitus was. Skillful argumentation will not always overcome doubt, since the work of only one of these predecessors, Velleius Paterculus, survives. Nonetheless, Eralda Noè argues a strong case, which has two main theses: that there existed from earliest imperial times traditions both favorable and unfavorable to Augustus and his successors and that Tacitus followed these traditions, thus not being the first historian to present Augustus in an unfavorable light.

There are two large rubrics to this study: the historiography of the civil wars and that of the empire. Among the subjects considered are Augustus himself, the relationship of Caesar, Pompey, and Cato, the civil wars, Augustus and the Greeks, the myth of Cicero, the elder Seneca, Cremutius Cordus, Aufidius Bassus and Servilius Nonianus, the tradition dealing with Tiberius, Velleius Paterculus, and the pro-Tiberian tradition, the Julio-Claudian dynasty, Cluvius Rufus, Josephus on Gaius and Claudius, the Flavian period, and the elder Pliny. This is an extensive conspectus. Noè is acquainted with modern scholarship, is judicious in her use of material and her conclusions, and steadily focuses on the seeming self-development of her theme.

Readers who wish information and discussion on this important subject may also wish to consult Ronald Syme's *Tacitus* (1958) chapters 22 and 23, Friedrich Klingner's "Tacitus und die Geschichtsschreiber des 1. Jahrhunderts" *Museum Helveticum* (1958): 194–206, Ronald H. Martin's "Tacitus and his Predecessors" *Tacitus*, edited by T. A. Dorey (1969) pages 117–47, and John Wilkes's "Julio-Claudian Historians" *Classical World* 65 (1971–72): 177–92, 197–203. Noè well merits inclusion in this distinguished company.

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STEVEN E. SIDEBOTHAM. *Roman Economic Policy in the Erythra Thalassa, 30 B.C.–A.D. 217.* (Mnemosyne, Bibliotheca Classica Batava, supplement, number 91.) Leiden: E. J. Brill. 1986. Pp. xi, 226. $38.75.

The "Erythra Thalassa" means the "Red Sea" if literally translated, but, in ancient sources, it can designate the Persian Gulf, the Indian Ocean, the modern Red Sea, or a combination of the above. Steven E. Sidebotham includes an appendix in his book that surveys the meaning of the term from the fourth century B.C. to the second century of our era and proves that it was used loosely, if there are any doubters. Thus, his study of Rome's policy deals with trade between the empire and the east, particularly with south Arabia and India, and explores the sources. In the end it is not clear that Rome had a policy, at least not a consistent one, but, as a compilation of information, Sidebotham's study, which is a revised version of his doctoral dissertation, is a valuable contribution.

The trade was extensive, although limited to products from which good profits could be realized, which was true of most of the trade within the empire as well. The chief imports from south Arabia were frankincense and myrrh, but there were also precious stones, spices, and sugar. Exports were ceramics, cloth, wheat, and sesame oil but not, it seems, Roman coins. If the Romans paid for these imports with gold and silver, as they did in India, they must have used bullion, for coins are not found on south Arabian sites. However, further excavation in the Yemen may change this picture. Imports from India included pepper, cinnamon, exotic fauna, cotton cloth, and some slaves, and exports included wine, with Italian brands preferred, copper, tin, glass, slaves, and coin. Sidebotham cites a Tamil poem of ca. A.D. 150 that mentions Yavanan (that is Roman) merchantmen that "arrive with gold and depart with pepper" (p. 23). Roman coins are found in great numbers in south India, although they are much rarer in the north. Until the reign of Nero, the coin of choice was the Roman silver *denarius*, but after Nero debased it to about 90 percent silver, the preference switched to the gold *aureus*; the debasement was the probable reason, though perhaps not the only one. But much trade was carried on by barter, and it is not clear that Rome suffered a severe deficit, as most scholars have assumed. As time went on, Rome reduced its dependence on the Red Sea trade for some exotic products it needed: pepper was grown in Italy and Egypt in the first century, and frankincense was produced in Syria a century or so later. Christian rituals were probably less lavish in their use of aromatics than their pagan counterparts, and the spread of Christianity probably reduced the market for myrrh and frankincense.

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Sidebotham devotes a separate chapter to the trade routes, in which he puts together a valuable collection of information on the Red Sea ports and the connecting roads. Another chapter deals with regulations, tariffs, and traders. A final chapter reviews Roman policy from Augustus to Caracalla. Augustus dispatched Aelius Gallus on an abortive expedition to south Arabia, and Trajan annexed Nabataea, thus making land communication between Egypt and Judea via Sinai possible without passing through foreign territory. But Rome had no consistent policy, except, perhaps, to maximize its profits, and this chapter is interesting but inconclusive. A last chapter sums up briefly.

The importance of the Red Sea trade should not be exaggerated. Egypt and Judea profited from it, but their prosperity did not depend on it. (The prosperity of Nabataea did, but it was a small part of the empire.) But Sidebotham has produced an important work. He has surveyed both the classical and the Indian sources, checked the archaeological data, and provided a valuable handbook for anyone interested in Rome's contacts with the east.

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MEDIEVAL

MARTYN J. WHITTOCK. *The Origins of England, 410–600*. Totowa, N.J.: Barnes and Noble. 1986. Pp. xi, 273. $31.50.

Although historical scholarship is always in flux, that of English history from 410 to 600 A.D. is subject to modification by virtually every archaeological spadeful. Although one cannot expect a revolutionary Sutton Hoo at every dig, the complexity of accruing evidence has tended to postpone attempts at summary of the period. There are many specialist studies, and both earlier Roman Britain and the later conversion period have seen several excellent recent surveys, but Martyn J. Whittock's volume has few rivals in attempting a summary of his centuries. Analyzing more than the research and opinions of others, he does not hesitate to take controversial stands on complex problems, while always considering alternative views; students of all levels will be grateful for this cohesive, stimulating account of the period's problems and the present state of knowledge.

After attacking "the Dark Ages" as "a defeatist term," Whittock searches for "the origins of the English" (a term for which he argues in preference to "Anglo-Saxon") not only in the well-known literary sources (Tacitus, Ptolemy, Bede, *Widsith*, and others) but in the light shed by archaeological evidence on third- and fourth-century folk movements, on pre-449 Germanic and Romano-British relations, and on the English attacks and British recovery. Chapter 2, "Romans and the English," discusses the famous "continuity problem," fully aware of the evidence for "something of an intellectual revolution" supporting continuity in both town and rural life but also fully aware of the problems. Here, as in the discussion of the "fire and sword" myth, place-name and linguistic data are used well and at length. Chapter 3, "English Society," traces the tribally mixed nature of the settlement, the complexities of authority structures among the invaders (with analytical use of surviving king lists), and Anglo-Saxon settlement, society, and life. Maps and tables are fruitfully employed to relate settlement names to political process. The final chapter, "The Founding of the Kingdoms," studies the expansion and consolidation of English power, region by region. Appendixes on genealogies of the early dynasties, the Tribal Hidage and its survival in place-names, and a "diagrammatic representation of early English personal names" conclude the volume.

Sober judgments combine with the personal enthusiasm (perhaps shown in Whittock's passion for exclamation marks) of one who has "lived" his field. Mysteries remain mysteries, but scholarship is combined with "the disciplined exercise of the imagination under the constraints of the available evidence" (p. ix). Whittock's methods range from his own field research and place-name studies to social science models and twentieth-century Cumbrian sheep-counting systems. Riches abound: for example, speculation on why farmers move, new evidence for the early existence of the black rat in England as cause of the sixth-century plagues, the possibility of Cerdic as a Jute.

In such a problem-laden field questions will also abound: for example, on his rejection of Vortigern as Gildas's "proud tyrant," his dating of Gildas, his conclusion that Oisc and Octha were "two separate people." His argument for the continuity of nucleated villages rests largely on nine "may haves" and "would haves" on one page (p. 74). An enthusiasm for sentence fragments and a failure to master the comma do not facilitate reading, nor do "Forth of Firth" and "Anglican" (for "Anglian"), among two dozen other slips—and at least this reviewer wishes he would discover the customary use of the hyphen ("Anglo Saxon," "quasi habitative," "proto Anglian," and so on). However, these must not cloud the outstanding contribution Whittock has made to the scholarship of early England. "Now is the time to bring these great currents of thought together under one heading" (p. viii), and Whittock has done it.

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J. A. BURROW. *The Ages of Man: A Study in Medieval Writing and Thought.* New York: Clarendon Press of Oxford University Press. 1986. Pp. ix, 211. $39.95.

The first part of this study categorizes the ways in which medieval writers imagined the number, length, and characteristics of the successive stages of human life. J. A. Burrow distinguishes the numerous schemes by their conceptual base rather than by the number of stages posited. Chapter 1 examines schemes that explained the ages in terms of natural causes. A medical model, for example, posited four ages parallel to the four humors; an astrological model posited seven ages parallel to the seven planets. Chapter 2 turns from the writings of scientists to texts by preachers, exegetes, and historians. Here Burrow finds a similar impulse to see the life of an individual as a microcosm of God's larger scheme, but in these writers the parallel was not to the physical world but to divisions in time. Thus, seven ages paralleled the seven canonical hours, or six ages paralleled the six ages of the world as laid out by St. Augustine. Four main traditions specified either three, four, six, or seven ages, but there were lesser traditions with different numbers, and there was variation within each tradition in naming the ages and defining their chronological limits.

The second part of the book takes up the question of how these norms were used in the treatment of individual cases in medieval narratives. What were the ethical implications of either conforming or not to one's particular stage in life? Burrow distinguishes two types of ideals: transcendence and nature. The ideal of transcendence found virtue in the individual who transcended natural age in demonstrating virtues characteristic of another stage, for example the *puer senex*, who avoided childish games and had the wisdom of an older person. But transcendence was for the few, such as the saints, who commonly attained this ideal. For the many virtue was to be found in following the course of nature. Thus, the *senex amans*, a person whose amatory activities continued beyond the period set by nature, was an object of mockery and scorn.

This study will be useful to anyone interested in placing a particular reference to the ages of man in the framework of the variety of medieval schemes. The book also raises briefly a number of larger issues of intellectual and cultural history that merit further consideration, for example, the lack of concern with the disjunctures between a particular scheme and observable human experience and the easy acceptance of the coexistence—even harmony—of various schemes that to a modern reader seem mutually exclusive.

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MARTHA C. HOWELL. *Women, Production, and Patriarchy in Late Medieval Cities.* (Women in Culture and Society.) Chicago: University of Chicago Press. 1986. Pp. xv, 285. $25.00.

This book is the work of a true scholar. Martha C. Howell proposes to tell what kind of occupations women held, the limitations imposed on their work, and the gender and economic systems they faced in the late Middle Ages in North European towns. In particular she researches Leiden, an important maker of heavy cloth of English wool, called drapery, and Cologne, a significant merchant-producer of metal, silk, gold embroidery, cloth, and spices. Leiden's old patrician elite prepared drapery from 1430 to 1590, and in Cologne a new rich social class descending from Jews and imperial administrators controlled the economy and politics after 1396.

Women in Leiden, according to Howell, could enter into dying and finishing drapery but were excluded from weaving and fulling. They could sell good quality cloth for the same price as men and could also engage in linen weaving and producing crude wool-blend cloth. The population of Leiden was approximately 14,000 in 1498. We can assume that 2,400 women worked, with about 1,500 being domestic helpers. Of the men 4,553 were employed in nondomestic work; thus, only 17 percent of the nondomestic labor force were women. Not more than 100 women were dyers, and 100 more did textile-related work, so that altogether 200 women, less than 10 percent of the total female workers, were occupied in cloth-related industry.

The author observes that in the suburbs of Leiden women made up 42 percent of the work force, and 68 percent of the female population was concentrated in textiles. In addition, they made pots, ran bakeries, operated pubs, and engaged in innkeeping, clothmaking, and retail merchandising of edibles, farm products, and items for the household. Suburban women around Leiden were more likely to work in industry and commerce than urban women.

In Leiden as well as in the suburbs, married women and widows had a higher economic and labor status than spinsters. From this Howell concludes tentatively that marriage enabled women to enter the market, in their husbands' lifetime, in middle-income jobs. Unambitious, unhealthy, unintelligent, and unattractive women never married and had low-status jobs, if any.

Cologne had a population of thirty thousand to forty thousand, and its artisans produced chiefly for export. Cologne's artisans belonged to guilds, and, of these, three were restricted to women: the guilds of the yarnmakers, goldspinners, and silkmakers. Women could also be equal partners with men in baking, linen weaving, silk embroidery, brewing,

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needle making, and belt producing. In many instances they shared this work with their husbands. Thirty-six other guilds, however, prevented female participation. Members of the women's guilds owed their participation to fathers or husbands who were silk traders, gold beaters, or yarn merchants. As mistresses in the guild, they did not have voting rights. Also, the guilds that women and men could enter had "little political status." However, women did engage in the export-import business and retail trade. Many were the agents of their husbands or heirs. They were the wives not of capitalists but of craftsmen or were themselves small artisans.

Surprisingly, the husbands were not the wives' partners in trade. Close study proves nevertheless that the family enabled the entrance of women into high-status occupations. To be sure, some unmarried women also held high-paying jobs, but their positions were attributable to wives who worked as their husbands' partners and created a sympathetic environment for spinsters.

In Cologne the capitalistic society permitted women to hold high-level jobs; in Leiden the noncapitalistic drapery production offered them middle-level work. But, in Leiden in the sixteenth century, the crafts came to regulate production and set the requirement for training members. The more political these crafts became, the more they opposed women. Only men could participate in government, bear arms, and uphold the work ethic. Thus, by the end of the fifteenth and increasingly in the sixteenth centuries, the crafts began to attack family enterprise. Women thus had to give up their middle-income jobs, become housewives, or occupy low-paying jobs that were beneath their social status. In Cologne a similar situation prevailed. Late in the fifteenth century, as capitalism improved trade and the guild limited the woman's right to help in the husband's shop, women began to restrict their participation to the commercial aspects of business, concentrating on local and regional transactions. Thus, in Cologne more women were able to remain in middle-level work.

As convincing as Howell's study is, her conclusion that women tended to become excluded from business or were slipping to a category below their social standing needs modification. By 1600 Leiden was a Dutch, Calvinist town, and the ruler added to his power by controlling religious affairs. Cologne remained a Catholic town. Could not these factors have added greatly to the difference between the two towns? Furthermore, Protestantism encouraged the growth of capitalism in Holland. Did this contribute to closing middle-level jobs to women?

I have two minor objections to the book. Too many German and Dutch terms are untranslated. Furthermore, theories appear that are not always fully developed until later in the volume. But these are minor criticisms in a satisfying book.

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MODERN EUROPE

LINDA FREY and MARSHA FREY. *Societies in Upheaval: Insurrections in France, Hungary, and Spain in the Early Eighteenth Century.* (Contributions to the Study of World History, number 6.) Westport, Conn.: Greenwood. 1987. Pp. xii, 142. $29.95.

J. H. SHENNAN. *Liberty and Order in Early Modern Europe: The Subject and the State, 1650–1800.* (Studies in Modern History.) New York: Longman. 1986. Pp. xii, 144. $10.95.

In 1974 J. H. Shennan published a stimulating little book, *The Origins of the Modern European State, 1450–1725*, in which he traced the intellectual and institutional factors that went into the making of the modern political framework: the personal authority of the prince developed into the impersonal state that has dominated the life of the Western world since the eighteenth century. In the book under review Shennan takes another step in further explicating the patterns and tensions that determined the political evolution of Europe. He analyzes both the intellectual and institutional aspects of the antinomy produced by the need for security (or order) guaranteed by the impersonal state and the urge for liberty on the part of the individuals for whom the state exists. He argues that the need for security or order was (logically and temporally) prior and that, once established, it generated the aspiration to eliminate the personal authority of the ruler and the society of "orders" (Roland Mousnier) that it implied. This in turn impelled the drive for individual liberty—a liberty that, if implemented in full, threatened the preservation of security. A compromise had to be found by which the relationship between the impersonal state and the free individual would be mediated by laws equal for all within the national bond.

Shennan illustrates and argues this thesis on the basis of the opposite examples of France and Russia, touching in passing on Spain and England, and on Poland, Sweden, and Prussia. His argument is that France under the *ancien régime* did not manage to eliminate the monarch's personal authority so that a society of orders lingered on and the impersonal state could not be firmly rooted. As a result, individual liberty and social order were not harmonized and the ensuing strains led to revolution. In Russia, on the other hand, Peter the Great tried to replace

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the tsar's patrimonial authority, sanctioned by the church, with a bureaucratic state machine on the Western model. He largely failed because Muscovy had not produced the *société d'ordres* that was a prerequisite to overcoming the total freedom of the Cossacks while preserving the country's security from foreign threats. Catherine II fostered the creation of the social order that would be the *interlocuteur valable* with the ruler and enable the latter to promote security as well as the rule of law. But this caused the more enlightened (that is Westernized) sectors of society to become advocates of full individual liberty, an advocacy that led to the radical demands of the late eighteenth and the revolutionary ferment of the nineteenth centuries. The reason for this paradoxical result was that only the ruler's personal authority had enabled the establishment of social estates and it had to be preserved to maintain security and order.

The thesis and the argument are both original and suggestive. Naturally, Shennan is at his most convincing in the case of France, whose history he knows thoroughly. His knowledge of Russian history is a bit less secure, so that the specialist will question some generalizations on which he bases his arguments. But this is of secondary importance. What is truly important is that Shennan looks at the well-known history of France and Russia from 1650 to 1800 with fresh eyes, from a perspective that converges with the conclusions reached by recent works on the history of France, Germany, and Russia that free us from the strait jacket of irrelevant conceptual categories (class) and explanatory schemes (whig teleology), while relating the patterns of social and political evolution to efforts at disciplining and transforming the ways of "traditional" populations. It must be confessed, however, that Shennan has not quite freed himself from using England's experience as an implicit yardstick and from seeing the triumph of political "modernity" as well-nigh foreordained, at least as far as Europe is concerned.

One would have hoped that the monograph by Linda Frey and Marsha Frey would provide an illustration of the violent struggles that accompanied the process described and so felicitously analyzed by Shennan. They occurred at roughly the same time, and each is an instance of the resistance offered by established social structures and "traditional" ways to the efforts at enforcing a more "modern" political order, which also happened to be of foreign origin. Unfortunately, the authors of this slight monograph become mired in a critical discussion of sociological models and political science interpretations that are best relegated to oblivion. And when they come down to describing and analyzing the three rebellions of their concern—the *Camisards* of the Cévennes, Ferenc Rákóczi II, and the Aragonese in favor of Charles of Austria—they do it at such a superficial level that the reader obtains neither new factual information nor suggestive interpretations. Also, there is no meaningful overall conclusion that might help place these revolts within the specific context of eighteenth-century European history.

MARC RAEFF
Columbia University

ROGER CHARTIER, editor. *Les usages de l'imprimé (XV^e^–XIX^e^ siècle)*. Paris: Fayard, with the cooperation of the Centre National des Lettres. 1987. Pp. 446. 150 fr.

Roger Chartier has gathered an intelligent and interesting mélange, based on seminars at the Ecole des Hautes Etudes. The studies in this volume, all concerned with the role of printed works in the political and religious culture of early modern Europe, are emblematic of the movement of social science history from the sociological, aimed at wide generalizations based on statistical movement, to the anthropological, the close study of individual cases in their symbols and meanings. Chartier correctly identifies three concerns that join the otherwise diverse works: close attention to printed materials other than the book, such as the *livret*, the *placard*, and marriage contracts; concentration on individual case studies, such as the single event or the single small work; and scrupulous attention to the ramified contexts in which the works were produced and read.

Within this framework the reader confronts a very varied menu of uniformly intelligent studies. Alain Boureau offers two studies: the first concerns the essential continuity of ecclesiastical politics embedded in short saints' lives in the Franciscan context from the High Middle Ages through the seventeenth century; the second studies the growth of emblem books and their transformations in the seventeenth century. Chartier presents two essays: a close study of the social and cultural purposes embedded in a few *livrets*, depicting the miraculous salvation of an unjustly hanged woman, and the role in the seventeenth century of *chartes de mariage* in preserving and enhancing the meaning of the marriage ritual. Catherine Velay-Vallantin shrewdly explores the mutations of the tales of Perrault as the larger work was broken down into individual *livrets* and the stories themselves subjected to moralizing commentary and tendentious illustration. Paul Saenger depicts the role of the "book of hours" in reinforcing and promoting what he considers a revolutionary change to an emphasis on private prayer after the thirteenth century. Marie-Elisabeth Ducreux surveys the way in which the trade in *livrets*

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underwrote the growth in heresy in eighteenth-century Bohemia. Christian Jouhaud offers two essays on political imagery in the seventeenth century: on the role of *placards politiques* in seventeenth-century political propaganda and on the construction of books and *livrets* centering on the triumphal royal entrée after the defeat of La Rochelle in 1628.

It is difficult to generalize from such a diverse volume. Three observations seem worth making. First, when one moves from broad sociological generalization to close contextual study, the role of the "print revolution" in Western culture becomes more complex and harder to identify. Rather than causing specific changes, the role of movable type and the press in these case studies seems to be that of operators that enhance certain movements and tensions already existing in the society, such as the movement toward private prayer, the use of hagiographical literature for ecclesiastical propaganda, or the growth of the use of emblems and symbols to enhance royal stature. Second, the studies emphasize what earlier work on the *bibliothèque bleue* has already shown, that is, that we make a mistake in assuming that the print revolution adhered to a privileged reading class but rather that we must pay attention to the multiple audiences of a medium that could produce works from the most magisterial folio volume to the single-sheet, wholly visual *placard*. Third, the studies emphasize the role of printing in expanding the access to images as well as words.

HARRY C. PAYNE
Hamilton College

ARTUR ATTMAN. *American Bullion in the European World Trade, 1600–1800*. (Acta Regiae Societatis Scientiarum et Litterarum Gothoburgensis, Humaniora, number 26.) Göteborg: Kungl. Vetenskaps- och Vitterhets-Samhället. 1986. Pp. 115.

On balance, studies of the world economic system in the *ancien régime* have been more theoretical than empirical. Artur Attman, however, is one scholar who has followed an empirical path to establish more precise patterns of world-wide bullion and trade movements during this epoch. These include his studies of Baltic markets, of bullion flows between Europe and the Far East from 1000 to 1750, and of Dutch participation in the world bullion trade from 1550 to 1800. His new study analyzes the role played by American gold and silver in world trade in the seventeenth and eighteenth centuries. For Spain he shows that Cádiz played a vastly more important role after 1650 as the transshipment point for American bullion to other parts of Europe, the siphon for funneling specie to the money markets of Western Europe. In the eighteenth century shipments of Brazilian gold to England via Lisbon fueled the world economy and became more important than Spanish-American silver as the source of bullion for England. For France remissions of American pesos from Spain was a way to balance its ever-present trade surplus with Spain and to make up for the chronic shortage of coin from French mints. Throughout the seventeenth and eighteenth centuries, most West European countries had a favorable trade balance with Spain, but their commercial balance with the Baltic nations, the Levant, and the Orient was unfavorable and had to be made up with American bullion. Attman shows also that the flow of European-American bullion to the East more than quadrupled over the seventeenth and eighteenth centuries—from 4.4 million rix-dollars annually in 1600 to 18 million in 1800—in much the same pattern as it did in the Baltic. Attman emphasizes too the ingenuity of the Dutch in serving as agents for English commercial transactions world-wide long after England had become Europe's pre-eminent international trader.

The author uses a wide variety of published sources in over ten languages to fashion his lean, tightly framed, descriptive analysis. When his sources diverge, the author lays them all before the reader, leaving one to decide on those data that might be accorded more critical treatment. Extremely useful are Attman's conversion tables for the various European and American monies of the Old Regime and his excellent bibliography. Students of the world economic system should welcome this study as an invaluable compilation of data showing world-wide bullion movements and their linkages to trade during the early modern epoch.

JOHN JAY TEPASKE
Duke University

ALAN PALMER. *The Banner of Battle: The Story of the Crimean War*. New York: St. Martin's. 1987 Pp. xiii, 289.

So much work has appeared on the Crimean War in the last fifteen years that the present need is synthesis. Alan Palmer gives us a lively, readable account that goes a long way toward fusing new research into the military events of the war, although it is less successful in integrating diplomatic history.

Palmer's greatest achievement lies in juxtaposing accounts of the war from the Russian and allied points of view in contrapuntal fashion, demonstrating a fine sense of irony. For the Russian side he depends particularly heavily on Albert Seaton (*The Crimean War* [1977] based on the multitude of participants' accounts published in Russian journals after the war) and on John S. Curtiss (*Russia's*

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Crimean War [1979] based on archival research in Moscow). He interweaves these comparatively new findings for the first time with the familiar story of French and British experiences, which, however, he has expanded on the British side with new documentation from the letters of Colonel Alexander Gordon to his father, the prime minister, and with fuller use of the letters of Fanny Duberly and her husband, Captain Henry Duberly. He is least novel on the French struggle, on which little has been done since Brison Gooch's classic *The New Bonapartist Generals in the Crimean War* (1959), although his judicious mining of the papers of General Hugh Rose, British liaison officer to the French, gives some additional perspective. He is also scrupulous about telling us what little can be learned from Western sources about the Ottoman war effort, although he does not include an account of the siege of Kars. And he brings out the many innovations on both sides in technology and in nursing.

Palmer's strategy is notably effective in showing how each side floundered. But perhaps his concentration on the immediate detail of the campaigns favors the Russians unduly. It could come as a surprise to the reader when the Russians, after a remarkably orderly evacuation of one naval base on the periphery of their empire, accept defeat. This seeming anomaly has been addressed and explained through more attention to the Russians' larger strategic problems by Winfried Baumgart in *The Peace of Paris 1856* (English edition, 1981) and by Norman Rich in *Why the Crimean War?* (1985). And Palmer is weak on the diplomatic context. Apparently discounting Curtiss's treatment, he shares the paranoia about Russian intentions prevalent in his British sources. He is shadowy on French objectives and inadequate on Ottoman policy. And, virtually innocent of German-language texts, he is largely unaware of recent reevaluations of Buol's role: Paul Schroeder, less positive than many, gives a good introduction to this problem in *Austria, Great Britain, and the Crimean War* (1972). But these failings are relatively minor within the scope of his work. It is refreshing to see diplomacy given this much attention in a modern military account; it is particularly encouraging since this is the most up-to-date and comprehensive military account presently available in English.

ANN POTTINGER SAAB
University of North Carolina, Greensboro

CHRISTIANE EISENBERG. *Deutsche und englische Gewerkschaften: Entstehung und Entwicklung bis 1878 im Vergleich.* (Kritische Studien zur Geschichtswissenschaft, number 72.) Göttingen: Vandenhoeck und Ruprecht. 1986. Pp. 391. DM 75.

Christiane Eisenberg's comparison of the origins of German and English trade unionism is a welcome addition to a much worked field. Eisenberg centers her analysis on the differing legacies bequeathed to unionism by handicraft movements. English unionists tended to benefit from the accomplishments of artisanal forerunners, she argues, whereas German organizers generally did not. Eisenberg interprets these contrasting handicraft traditions as the result of divergent political and market conditions in the two countries.

England's unique island geography, early national unification, and capitalistic development with its attendant separation of masters and men facilitated the growth of relatively free labor markets. Government policies reinforced these trends at the expense of guild institutions that had previously restricted the mobility and independence of workers. As a result, employers and organized journeymen faced one another as bargaining partners as early as the 1720s. The "friendly societies" of poor masters and journeymen not only negotiated wage settlements for dues-paying members, occasionally with strikes and court settlements as leverage, but also developed impressive benefit programs. The local, trade orientation of the older handicrafts outlived the collapse of the friendly societies in the 1820s, undermined efforts to establish national, federated unions in the 1830s, and distracted workers from Chartism in the 1840s. The "new model unions" founded in the 1850s finally managed to overcome this parochialism but owed much of their success, nevertheless, to the friendly society tradition of serving members with useful programs. After the enfranchisement of workers in 1867 came political clout for labor, improved legal status for unions, and recruiting breakthroughs.

Based largely on recent monographic literature and some primary sources, the sections of the book on England serve mainly to place in perspective the more solidly researched passages on Germany. Vastly different conditions prevailed in the northern reaches of the Holy Roman Empire. Political and economic fragmentation delayed the spread of capitalism and free labor markets in Germany until the onset of the industrial era. In great contrast to English policy, moreover, German states reinforced the privileges of master craftsmen. Consequently, the eighteenth and early nineteenth centuries witnessed a steady deterioration of journeymen's associations and a tendency toward radical political solutions of workers' problems. Leftist politicians also founded the first unions during the late 1860s but lacked the rich organizational heritage and practical experience of unionists across the Channel. Political authoritarianism after 1871 strengthened the radical political component of German unionism.

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Eisenberg's monograph is a threefold revision of the literature. The central thesis is a major challenge to historians such as Ulrich Engelhardt and Rudolf Boch who argue that German unions emerged "from below" out of the guilds. She demonstrates persuasively that the traditional tailors', cobblers', hatters', and glove-makers' guilds actually represented an antiunion force by the 1860s, whereas prounion handicraftsmen emerged only from newer trades such as printing, cigar making, and ship building in which capitalistic conditions prevailed. Second, Eisenberg's comparison of varying labor markets and guild traditions shifts attention to differences between England and Germany that are underemphasized in standard histories such as David Landes's *The Unbound Prometheus* (1969). Although her claim is true that England's lead in mechanization and steam power is often exaggerated—and the lengthy survival of handicraft overlooked—the island nation nevertheless possessed a highly articulated "technological system" by the 1820s that was not matched on the Continent. For these differences Eisenberg should have consulted Bertrand Gille's *Histoire des techniques* (1978).

Lastly, Eisenberg agrees with Gerhard A. Ritter and Klaus Tenfelde that political parties founded the first German unions in the late 1860s but does not see this as "a mistake at birth." Given the absence of strong support programs, less emphasis on ideology would have delayed unionism in Germany and weakened unions in the face of early storms. Although all this seems plausible, it begs questions about the long run. Whatever services socialism, liberalism, and Catholicism may have offered in the early years, these ideologies played a divisive role in succeeding decades, weakening unions before the final fury in 1933. Such divisions were probably inevitable, but historians should recognize these as a bane, not a boon.

ERIC DORN BROSE
Drexel University

ADAM PRZEWORSKI and JOHN SPRAGUE. *Paper Stones: A History of Electoral Socialism*. Chicago: University of Chicago Press. 1986 Pp. vi, 224. $24.95.

The subtitle of this slim volume, "a history of electoral socialism," is likely to be considered inappropriate or even deceptive by most historians. Adam Przeworski and John Sprague are not historians, nor is this a work of history. It is a social-scientific study of electoral socialism, using a grab-bag of historical examples. Historical context and evolution, the transformation of electoral issues over time, are not the main concerns of this "history."

The problems addressed are familiar to historians of Europe. Many socialists by the latter half of the nineteenth century came to believe that socialism could be reached through democratic elections to parliament. That belief in turn was based on the expectation that the working class would, in the near future, constitute a decisive majority of the population. Workers would use their votes as "paper stones" (an arresting image for a title, if not a very satisfactory one, since the socialist ideal was to build on capitalism, not destroy or "stone" it). The dilemma was, as the opening sentence of the book states, that "no political party ever won an electoral majority on a program offering a socialist transformation of society." Workers never became a majority in any European country.

The discussion of these and related matters in the first fifty to sixty pages of the book is, for the most part, clear and straightforward, although, again, it offers little if anything novel to historians familiar with the period. The problems lie elsewhere.

The authors state in their prologue that "our central thesis is that the voting behavior of individuals is an effect of the activities of political parties. More precisely, the relative salience of class as a determinant of individual voting behavior is a cumulative consequence of the strategies pursued by the political parties of the Left" (p. 9). In case such sentences leave the reader uncertain about what is being discussed, mathematical formulas are offered as "clarifications." We are able to see more clearly, for example, on page 64, where the clarifications begin, that "the change in the socialist vote between two consecutive elections" comes down to $\Delta W(t)=p[X(t+1)-W(t)]-dN(t)$. Of course, this is only an elementary example. By pages 140–41 such clarifications at times fill most of the page; one of the few nonalgebraic passages there informs us that "since both the objective function and the constraints are linear, the derivative of the Langrangian with regard to the strategic variable will be either positive or negative" (further mathematical formulas follow).

There may be some professional historians who will be able to read this volume with understanding and profit, but they will be rare. Since it is hard to imagine that any lay reader of history will profit from it at all, one wonders to whom the book is addressed and whether it might have been better placed, in an abbreviated form, in one of the social-scientific journals that cater to this sort of thing—especially since most of the intelligible parts of it merely go over familiar ground.

ALBERT S. LINDEMANN
University of California, Santa Barbara

ARCADIUS KAHAN. *Essays in Jewish Social and Economic History*. Edited by ROGER WEISS. Foreword by

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JONATHAN FRANKEL. Chicago: University of Chicago Press. 1986. Pp. xx, 208. $27.50.

Arcadius Kahan died in 1982, before completing his major work on the economic history of European Jewry. The essays in this volume, collected and edited by Roger Weiss, represent parts of what would have been Kahan's larger work. This collection of thirteen essays, three previously published, presents an intricate tapestry of twentieth-century European Jewry. The strength of the collection is that Kahan balances his own experiences as a Jew growing up in Europe with rigorous scholarship.

The essays follow a roughly chronological order. The first three are a demographic and socioeconomic survey of Russian Jewry in late imperial Russia, followed by two essays on the impact of Jewish emigration from Eastern Europe. The most interesting essays are accounts of interwar Vilnius and the Łódź and Warsaw ghettos. The three concluding essays discuss the problems of Soviet Jewry. There is one essay on Jewish life in the United States that is thematically inconsistent with the remainder of the volume and should have been omitted.

The premier essay, and the lengthiest, is a quantitative analysis of the impact of industrialization on the Jewish community in tsarist Russia. In his analysis Kahan uses descriptive rather than inferential statistics. The charts and graphs in the text and the appendix will be a valuable reference for others who continue the work Kahan began, but the narrative is cumbersome and confusing. There are clear contradictions and inconsistencies. In a discussion on the attitudes of Russian big business toward Jews, Kahan states, "We have no direct data that would reflect the attitude of 'big business,'" yet one paragraph later he states, "One could, therefore conclude that the attitude of big business toward discrimination against Jews . . . was by and large either not expressed or neutral" (pp. 38–39).

A major weakness in his analysis is the failure to consider correlations between the Jewish and non-Jewish populations. Although it appears that there was a dramatic increase in the birthrate in the Jewish population during the early twentieth century, there was a commensurate increase in the non-Jewish population. Kahan's demographic analysis also fails to consider intervening variables, such as pogroms, wars, conversion, and emigration, that influenced demographics.

Kahan's presentation and explanation of the data are particularly confusing. In discussing table 3 (p. 8), "Comparison of Census Employment Data and the JCA Data for Industry," the text indicates that the table shows four major branches of employment, although it actually shows six; table 5 (p. 18), "Jewish Membership in Upper Strata Groups," reports the number of Jews in upper classes by individuals, but the text reports the data by household. This is so confusing that even the explanatory footnote does not help. In table 8, "Value Output per Worker Employed," Kahan concludes that "the table reveals . . . production per worker was increasing inversely with the 'age' of the industrial plant" (p. 22), maybe so, but there is no evidence in the table for that conclusion. The more serious problem is that Kahan makes mathematical assumptions and computations without supplying sufficient information to reconstruct his calculations (notes 25 and 27 especially). Although the essay raises several legitimate points, a more judicious editorial pen was needed.

The strengths of the collection are the essays on interwar Vilnius and a moving social historical account of a day in the ghetto. In these essays Kahan does not let statistical information overwhelm his narrative. In a balanced presentation of data, personal insight, and anecdotes, Kahan takes the reader through a tour of his hometown. The development of Vilnius as a center for Jewish cultural and intellectual growth is represented in Kahan's semiautobiographical account, as is the tragedy of the destruction of this vital center of Judaism. Kahan's discussion of Vilnius gives life to the people and personalities. In discussing his days at Vilnius University Kahan agonizes over the systematic persecutions: "The price demanded for our education became excessive; it involved a denial . . . We were saddened and we cried, out of our helplessness and because we felt betrayed" (pp. 168–69).

In the closing essays on Soviet Jewry, Kahan indicates that the homogeneity of Russian Jewry is being crushed by Soviet authoritarianism. Before 1917 Russian Jews were located in one geographical region, the Pale of Settlement; now, authorities have consciously attempted to deny Jews "community" by dispersing them throughout the Soviet Union. Kahan concludes that, although this constitutes a threat, as long as the family unit remains intact the longevity of Judaism in Russia will persist.

This collection of essays would have benefited from more rigorous editing. Possibly, Weiss did not want to deviate from Kahan's original text, but in doing so he obfuscated the author's intent.

SHLOMO LAMBROZA
St. Mary's College

ALEXANDER J. DE GRAND. *In Stalin's Shadow: Angelo Tasca and the Crisis of the Left in Italy and France, 1910–1945*. Dekalb: Northern Illinois University Press. 1986. Pp. viii, 231. $27.00.

This book is not a biography of Angelo Tasca. Alexander J. De Grand explains: "The true subject of this book is the Italian Left (and to a lesser extent

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the French Left) from 1918 to 1945, studied through Tasca's activities" (pp. 6–7). Its value lies precisely in his analysis of the Italian Left and the role played by one of its lesser-known but not unimportant leaders. The ideological struggles among socialists and between them and the Communists are already well documented. Until the appearance of this monograph, however, Tasca's contribution, both to political strategy and to ideology, was obscure. One will search in vain for his name in general histories of collectivism; it never appears.

Tasca's public life—and his private life, about which we learn so little—was tragic, precisely because his most constructive years were spent "in Stalin's shadow." In 1921 he left the Italian Socialist party for the new Communist party (PCI). He rose in the hierarchy, along with Antonio Gramsci, but the two did not share the same views on labor unions and methods to oppose fascism. When Tasca, as the PCI's representative to the Communist International, sided with Nikolai Bukharin against Stalin, he was expelled from the party on Stalin's orders in 1929.

This event marked his life decisively. He rejoined the Socialist party while in exile in France and became increasingly anticommunist, distrusted Communists as allies in the struggle against fascists, and advocated more fervently the strategy he had put forward in the 1920s: a broad alliance of workers and petty bourgeois, sealed by a kind of broad humanist collectivism both anticommunist and anticapitalist. In the 1930s he fell increasingly under the influence of "neosocialists" who would collaborate with the Nazis during World War II. Unfortunately, Tasca, too, was pulled toward a form of collaboration when he worked as a journalist for the Vichy regime in France. His decision not to flee to England branded him as a traitor to the causes for which he had fought and suffered exile. There is a psychological issue here that De Grand does not seriously touch on.

And yet he has carried out extensive research in unpublished, published, and oral sources and has ably brought out Tasca's important contributions to party strategies and debates. Ideas became his stock in trade, especially after 1929 when he had to earn his living mainly in journalism and publishing. He was, with the benefit of hindsight, correct in his antifascist strategy and keen in his critique of Marxist philosophy as used by the Communists. He, of course, abandoned it in the 1930s. He was never successful, however, in convincing party leaders and was always the outsider in higher circles of leadership. His involvement in endless debates was unrelenting, and De Grand is as unrelenting in his descriptions of them, at times to the point of tedium. His decision to organize his material in strictly chronological order has too often led him to repeat himself. These weaknesses admitted, De Grand has written an important analysis and raised his fallen Angelo to a level somewhat higher than that of traitor.

LEO A. LOUBERE
State University of New York, Buffalo

J. M. BRERETON. *The British Soldier: A Social History from 1661 to the Present Day*. London: Bodley Head. 1986. Pp. viii, 208. $18.96.

To write a social history of the British army from 1661 to the present day would be a significant challenge to most historians. To attempt to do so in less than two hundred pages would seem to be an almost impossible task. But this is what J. M. Brereton, a retired British army officer, has set himself to do.

Beginning with the army of King Charles II following the Stuart Restoration, Brereton traces the evolution of the British army over some three hundred years, looking specifically at topics of interest to the social historian. His concern is with soldiers rather than officers and with life in the military, not military campaigns. For each period, whether it is Wellington's army or Montgomery's, similar subjects are identified and examined. From what elements of the population were the soldiers drawn, how were they recruited, and what were their terms of service? What were their living conditions, food, clothing, equipment, and housing like? What of medical treatment, punishment, educational opportunities, pensions, and welfare support organizations? How well were soldiers paid?

Clearly, the concept behind this study is sound. Unfortunately, the author's approach is not. Rather than attempting a true social history, employing the statistical, demographic, anthropological, and other research tools and methods that have distinguished more recent social historical research, Brereton has adopted the traditional, narrative approach in his book. It is apparent that, in researching this book, the author was primarily concerned with providing specific information about the care and maintenance of the troops rather than with analyzing the prevailing conditions and attempting to determine how and why these conditions changed over time. Topics are dutifully mentioned, occasional examples furnished, but in-depth analytical information is not to be found. The result is a sweeping, almost superficial survey that provides little in the way of new information and is certainly not social history.

Brereton is writing for a British audience. His pride in the accomplishments of the British army over the centuries is apparent, and his use of terms

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uniquely British occasionally gives pause. In those cases where he does provide examples to illustrate the living conditions of the soldiers at a particular time—pay and allowances, for example—the information is almost meaningless since no information on the purchasing power of the pound is provided.

Although there are a few tidbits to be found in this volume, those looking for an informative social history of the British army are bound to be disappointed.

GORDON C. BOND
Auburn University

C. G. A. CLAY. *Economic Expansion and Social Change: England, 1500–1700*. Volume 1, *People, Land, and Towns*; volume 2, *Industry, Trade, and Government*. New York: Cambridge University Press. 1984. Pp. xiv, 268; xii, 324. Cloth the set $69.00, paper the set $25.90.

Thirty years ago F. J. Fisher remarked that the economic history of Tudor and Stuart England was still a "dark age," and the same comment could have been applied to the social history of the period. Since then a renaissance has come to both fields, in which the formidable bulk of articles and books is daunting. Specialist and nonspecialist alike confront another difficulty in understanding the significance of this scholarship, which is that much of the important work lies in studies of particular regions. Several excellent shorter surveys have described the main developments in the new scholarship, but C. G. A. Clay's presentation is the first detailed synthesis to relate the work many historians have published. Each chapter in the two volumes can be read as a separate entity, but it would be a mistake to regard this book as a fragmented survey. Clay has presented this material as an overview of society, with a careful judgment of the various parts in a way that contributes an original statement. Volume 1 covers the demographic, agricultural, and urban history of the period, and volume 2 deals with industry, trade, and governmental policy. Each volume is available in paperback edition, as well as hardback.

Clay has been a contributor to volume 5 of the *Agrarian History of England and Wales*, and he brings his extensive knowledge of the land and rural population to bear as primary forces in his survey. Supply and demand are necessarily related to population growth, and this period saw an irregular pattern of demographic change. After 1510 the rate began to increase, and between 1550 and 1600 the population of England and Wales doubled. But the same advance did not continue during the seventeenth century, and from 1640 to 1690 population may even have contracted slightly. Nevertheless, from 1500 to 1700 the population increased from ca. two million to five million.

On the land a market-oriented economy became more and more common in certain areas. Enclosure, the expansion of new cultivated area through technology, and the lands from the monasteries made the more substantial of the peasant farmers—the yeomen—the beneficiaries of this change, to the extent that their incomes and attitudes became aligned more to the gentry than the smaller farmers. The growth of London and, to a lesser extent, new urban centers, created new markets for agricultural products. The dispossessed farmers who survived starvation as they relocated themselves elsewhere as laborers consumed less than they had on the land; the surplus saved went toward the new markets.

As constraints on the old peasant society began to erode because of the demands of the new commercial farming, some farmers began to specialize their products for local markets whereas others began to diversify. By 1650 a more varied agriculture had evolved that placed greater emphasis on both dairying and grain farming. On the whole the period saw abundant crop yields, which had a greater inflationary effect, Clay believes, than other factors, such as recoinage.

Joan Thirsk's Ford Lectures in 1975 illuminated a range of small-scale industrial activity previously unknown in the period, and Clay has related these changes to the shifts in agricultural society. Much of this industry was performed by part-time agriculturalists, but there was a growing trend for this labor to fall into the hands of full-time wage earners distanced from the framework of farming society. Important conclusions can be drawn about proto-industrialization. Much of what Defoe witnessed in his *Whole Tour* had begun long before. "A market for non-essential consumer goods had emerged which was far larger and much more broadly based in social terms than anything that had gone before, and although the stimulus this provided for the growth of native manufacturing was still small compared with what was to come, it was already a significant factor in the development of the economy" (vol. 2, p. 36).

Scholarly pioneers such as Clay have recognized that the late Tudor and Stuart periods can no longer be regarded as doldrums between the agricultural and industrial revolutions of the eighteenth century. Clay's book explains this transitional society in language that is free of jargon and broadly based in its orientation. Changes in fashion, in building, and in governmental patronage are all examined here with the sort of detail that excites one to read further. This book should be essential reading for graduate students and advanced undergraduates in American colleges and universities, as well as for other scholars, such as literary and political historians,

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whose research ignores these major themes of the period.

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G. R. ELTON. *The Parliament of England, 1559–1581*. New York: Cambridge University Press. 1986. Pp. xii, 399. $39.50.

With this deep, masterly piece of scholarship, G. R. Elton has fundamentally reoriented Elizabethan parliamentary history, providing both an interpretation and a method of investigation against which future studies of early modern English Parliaments inevitably will be tested.

In a series of addresses and papers published between 1973 and 1978, Elton outlined the argument of the present book by describing the function of the parliamentary "point of contact" between Tudor monarchs and the Tudor political nation. Contrary to the views of Wallace Notestein and Sir John Neale (views that had enjoyed the status of textbook orthodoxy), the history of Parliament was not a Whig's tale of the "rise" of Parliament and the progressive "evolution" of Commons' procedures, an evolution allegedly born of conflict between the queen and Puritan-minded M.P.'s, but the story of characteristic cooperation among the members of the parliamentary trinity of queen, Lords, and Commons, cooperation aimed at providing political stability, resolving problems, and meeting the needs, public and private, of sovereign and subjects. Near-sighted preoccupation with Neale's imagined opposition distracted attention from the real business of Parliament, the making of statutes. The present book sets out more fully than ever before precisely how those statutes came to be.

After surveying contemporary records and conceptions of Parliament (chaps. 1–2), Elton considers the "structure of business" (chaps. 3–6). In a brilliant piece of detective work, he clarifies the distinction between public and private bills and acts (a distinction peculiar to the English Parliament); the origins of bills, including those that can be traced to lobbies; procedures governing the starting, progression, and amendment of bills, as well as the giving of the royal assent; and the repeal of acts.

In six chapters (chaps. 7–12) Elton details the passage and failure of countless bills touching supply, queen and state, church and religion, the common weal (social reform, agriculture, manufacturing, trade, and shipping), the reform of the law, and private matters. The final section (chaps. 13–14) addresses the politics of parliamentary management, privilege, and free speech and the political character of parliamentary responses to the succession question and Mary Queen of Scots. Rounding out the book are an index of bills (arranged alphabetically by type under each of the seven sessions studied here), an index of acts (which includes references, session by session, to unpublished originals at the House of Lords Record Office), and a general index.

Much of this is demanding reading, best undertaken thematically by using the superb indexes as guides. In every chapter the judgments rendered usually persuade by the sheer power and sweep of the research; by systematically following "events in both Houses as bills turned into acts or more commonly failed to do so" (p. vii), Elton has laid down fortress-like foundations for his conclusions. If (contrary to Elton) the notion that, financially, a king should "live off his own" had not disappeared by 1530 (p. 155)—the earl of Warwick espoused it in 1551—parliamentary subsidies in peacetime had nonetheless become routine by 1581 (p. 168). The terms "privilege" and "liberties" were not synonymous (as the traditionalists would have it); in practice, privilege meant "freedom from arrest and nothing else" (p. 333). Neale's argument that parliamentary procedures "matured" during this period is unhistorical; rules of procedure signify none "of the politically important things that Neale and Notestein read into them" (pp. 90–91). Neale misread and misunderstood the pasquinade purportedly identifying a "puritan choir"; the evidence points rather to a (non-Puritan) committee formed on October 19, 1566 to approach the Lords on the question of the succession: "the puritan choir is a myth and should be removed from the annals of history" (p. 353). On the question of the succession, no independent initiative developed within the Commons; pressure for the queen to marry originated at court and in Council (pp. 361, 374). Since Parliament by itself "had neither power nor policies" (p. 377), "the customary concentration on it as the centre of public affairs is entirely misleading" (p. ix). If Parliament "did stop a powerful executive from becoming an absolutist monarchy, it did so not by forming a counterpoise to the Crown but by assisting the Crown in lawful government" (p. 378).

These and other conclusions may well invite attack. Doubtless some in time will be modified; few will easily be overthrown.

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MARK A. KISHLANSKY. *Parliamentary Selection: Social and Political Choice in Early Modern England*. New York: Cambridge University Press. 1986. Pp. xiii, 258.

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In this book Mark A. Kishlansky advances a trenchant explication and analysis of how men were chosen for Parliament in early modern England, or more precisely in the 101 years between 1584 and 1685. Along the way he provides an important new vocabulary for what is likely to emerge as another dimension of the debate on the political importance of early Stuart Parliaments. For Kishlansky there is little sense in talking about elections and political contests for parliamentary seats before 1640. His alternative construct, which he describes with impressive clarity, is a process of "unified choice" overwhelmingly based on social and personal considerations. By and large it was the magistracy in the counties and boroughs that saw it as their function to select a number of candidates equal to the number of seats for their constituencies. These nominees, drawn from the social elite who presented themselves or who were advanced by patrons, were offered to an electorate that, in turn, appreciated its role as one of affirming rather than choosing. Equally important was the belief that a failure on the part of the magistracy to winnow the field, or the failure of the electorate to record anything less than the unanimous endorsement of the magistrates' choices, was a breakdown of the system itself and brought with it the implied threat to an ordered society.

Everyone knew the rules, and for the most part everyone played the game accordingly. Those whose social credentials dictated their eligibility for the honor of a seat could usually be counted on to arrange matters among themselves and never to press their claims to the point of conflict, and, when the next step in the process became necessary, an efficient magistracy could normally compose differences so that a public contest was avoided. When all worked as it was supposed to, the electors assembled at the appointed time and place to give their collective and unanimous voice to the candidates before them. This is what Kishlansky describes and designates as a system of parliamentary selection through unified choice. It was not, nor was it ever meant to be, an election in the modern sense of the word. The notion of an electoral contest suggested an adversarial proceeding essentially abhorrent to communities intent on the preservation of hierarchical order and social harmony.

The outlines of the selection process and the social culture that underlay it are already familiar to students of the period. Kishlansky's contribution is new and provocative because of his assertions that national politics had little to do with the process before 1640 and that not until the later Stuart period do we begin to perceive anything approximating an electoral contest built around political issues. Kishlansky is tilting against what he calls "the Whig canon," especially the work of Derek Hirst (*The Representative of the People?* [1975]), which emphasizes the growing number of contested parliamentary seats in the pre–Civil War era and argues that these contests were increasingly about the realization of political objectives.

Kishlansky, although acknowledging the increase in contests across the seventeenth century, posits that it was not until after 1640 that the increase became numerically significant or in any measurable way political. Competitions in the early Stuart era were aberrational rather than normative, and, when they did occur, especially in the pre-1640 era, they were overwhelmingly likely to have been bitter and socially divisive attacks on an individual's honor and integrity rather than issue-oriented campaigns for parliamentary seats. It was not until Parliament became the engine of government in the period 1640–60 that a seat in the Commons came to be viewed as something significantly greater than an ornament of an individual's social and community status.

Hirst's methodological problem was that he needed to build much of his argument for an increase in the number of early seventeenth-century electoral contests on a range of assumptions that were all plausible but that he appreciated were nonetheless open to question. Kishlansky has questioned these assumptions and, in some ten circumstances, challenged the factual data on which the assumptions were based. Yet much more important is that Kishlansky has chosen to adopt a qualitative and contextual approach and not to attempt any quantitative analysis. The data, he suggests, are not sufficient to be meaningful. "Contest counting is a mug's game. The aggregates are too small not to be affected significantly by the chance survival of evidence" (pp. 140–41, n. 12). This assertion, too, is open to question. Despite his vigorous and vividly sustained argument, it is more than likely that Kishlansky's disregard of numbers, coupled with his impassioned and partisan downgrading of the place of politics in the pre-1640 process of parliamentary selection, will provoke as much as it will satisfy. Kishlansky's is the latest word. It is likely, however, not to be the last one.

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DONALD DEAN SMEETON. *Lollard Themes in the Reformation Theology of William Tyndale.* (Sixteenth Century Essays and Studies, number 6.) Kirksville, Mo.: Sixteenth Century Journal. 1986. Pp. 285.

The position of William Tyndale within the theological spectrum of reforming thought is a question that has puzzled critics for a very long time; even more problematic is the issue of Tyndale's sources

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for his ideas, his obvious partial debt to Luther but his divergence from some of the German reformer's characteristic opinions. This book is a study of the possible influence on Tyndale of John Wyclif and his followers. Donald Dean Smeeton endeavors to look at all aspects of Tyndale's theology but omits, reasonably enough, any detailed analysis of the possible debt of Tyndale's biblical translation to the earlier Wyclifite versions. Thus, after two preliminary chapters dealing with the state of criticism on Tyndale and current knowledge of Tyndale's life, there are considerations of Tyndale's view of Scripture, his soteriology, his view of the church and of secular society. Three appendixes deal with Tyndale's possible responsibility for the printing of Lollard tracts, a note on attitudes to the biblical Epistle of James in the Reformation period and before, and an example of Tyndale's translation from Luther.

Previous writers have suggested, often in passing and never with much solid evidence, the possibility of Tyndale's inheritance from earlier native English reforming ideas set out at the end of the fourteenth century by Wyclif and carried forward by the Lollards in the fifteenth and early sixteenth centuries. Smeeton's suggestion is that this inheritance was considerable and that it constitutes the essential background for an understanding of Tyndale's thought. The thesis is an important one, and one that calls for considerable breadth of expertise and considerable tact in argument. The strength of Smeeton's case lies in the numerous and striking parallels that he is able to bring forward between ideas and expressions in Tyndale and in Wyclifite texts. These parallels demand explanation: they seem too numerous and too close to be coincidental, and their frequent similarity in wording suggests direct transmission. Smeeton has read widely in published Lollard sources, and, although his material from Wyclif himself is perhaps more limited (and sometimes draws on misleading anthologies), this matters less since the verbal similarities suggest that any link between Tyndale and the Lollards is likely to be through vernacular sources. The handling of those vernacular texts is not impeccable: the words of the quotation on page 136 are declared immediately afterward to be those of Bede and Ambrose, not of Lollardy. Similarly, although it is Smeeton's misfortune that research on the last fifty years of Lollardy is only just coming into print, to imply (as page 184 does) that the Alnwick cases of 1428–31 illustrate this period is misleading. Smeeton is also inclined even-handedly to attribute equal authority to all secondary studies; this gets him into difficulties when these authorities do not agree and leads to unnecessary citation of outdated opinion.

Smeeton's claims about Tyndale's inheritance from a native English tradition of dissent are important, and it is perhaps a pity that he tried to cover so much in a single monograph. Theologians may dismiss the idea because Smeeton does not examine any major reforming issue in sufficient detail; historians may be dissatisfied because he does not face squarely the practical question of how Wyclifite texts could have been known to Tyndale in the 1520s. This is an argument that requires bold presentation if it is to succeed against traditional scholarly attention to Continental models. But Smeeton sometimes seems to back away from his own contention: thus, in the midst of drawing some striking parallels between Wyclifite observations on preaching and those of Tyndale on the same subject, the author observes "although Tyndale was not likely to have known any of the texts just cited, his views on preaching were similar to those held by the Lollards" (p. 165). If this is to be taken at face value (and nothing that follows suggests it is not), then the whole purpose of the book seems to fall to the ground. Maybe the point of this disclaimer, and others comparable to it, is that Tyndale learned more from conversation with Lollards than he did from their surviving writings, but this is never discussed and is in any event irrecoverable.

Smeeton's thesis deserves further and more detailed analysis. Any single section of his book demands a monograph in itself to explore the other possible sources for Tyndale's theology and language before his contention of Wyclifite derivation will command full assent. But the possibility of such a derivation cannot, after this study, be ignored; for this new direction students of the early sixteenth century must thank Smeeton.

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J. C. DAVIS. *Fear, Myth, and History: The Ranters and the Historians*. New York: Cambridge University Press. 1986. Pp. xiv, 208. $34.50.

In recent years several important books have appeared on the religious radicals of the English Commonwealth, and especially on that sect known as Ranters. Christopher Hill's now classic *The World Turned Upside Down* (1972) and J. F. McGregor and B. Reay's *Radical Religion in the English Revolution* (1984) deal with nearly all of the radical groups. Focusing primarily on the Ranters, however, are A. L. Morton's *The World of the Ranters* (1970) and two collections of essays and documents: Nigel Smith's *Collection of Ranter Writings* (1983) and the current volume under review.

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From the foregoing authors, J. C. Davis has created a Ranter paradigm or core. The leading characteristics of the core were antinomianism—both theoretical (faith by justification) and practical (profanity and adultery)—as well as pantheism—God as the author of sin or, alternatively, "sin becomes nothing, the negation of God" (p. 23). By testing the Ranter tracts against the core model, Davis eliminates the sensational Ranter fringe (messiahs and prophets) and three peripheral Ranters: Richard Coppin, George Foster, and Joseph Salmon. These steps leave three inner core survivors: Jacob Bauthumley, Laurence Clarkson, and Abiezer Coppe, whose major works are all anthologized in Smith, and who had very little in common.

If the Ranters had no homogeneous core of belief, then they were no sect at all. What, then, gave them coherence? Davis's answer is the "Ranter Sensation," derived from about fifteen prurient tracts, nearly all, ironically, reprinted in Davis's book; more serious accounts from contemporaries such as John Tickell, from whom Davis may also have devised his paradigm; and the pamphlet disavowals from Baptist, Muggletonian, and Quaker leaders, who wished to discipline the spiritual enthusiasm of their own followers. The "Ranter Sensation," Davis persuasively argues, produced the Ranter myth—"the burlesqueing of the deviant" (p. 97)—in that uncertain and anxious society of 1649–50. The Ranter myth was enhanced by "the fear of oppression from above," as exemplified in the Blasphemy Ordinance of 1650 and other sects' "fear of disintegration within" (p. 101).

It troubles Davis that most Ranter historians have accepted many parts of the Ranter myth based on sensational evidence and unreliable heresiography such as Thomas Edwards's *Gangraena*. Assessing the truth as revealed by true believers is always a difficult task even for the most uncommitted historians, but especially with the Ranters when important elements of the myth can be found in the writings and actions of its inner core. Davis is particularly critical of those left-wing historians, Hill and Morton, who, he argues, having rejected both the Levellers and Diggers as the true purveyors of the people's "revolt within a bourgeois revolution," mistakenly strayed "in some sense" into a right-wing Ranter myth projected by those more conservative pamphleteers "who wished to reassert religious control" over the radicals (pp. 126, 134).

Most appealing in this book is the insight that traditional sectarian labels may impede the conceptualization of cross-relationships between radicals. For example, Davis suggests that Coppe had much more in common with the Leveller William Walwyn and the Digger Gerrard Winstanley than with the Ranter inner core. Therefore, the possibility exists of constructing a different typology of parts of radical Puritanism that links the radicals in new relationships often stemming from the antinomianism and millenarianism they heard preached in the New Model Army. But that is another book.

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C. W. BROOKS. *Pettyfoggers and Vipers of the Commonwealth: The "Lower Branch" of the Legal Profession in Early Modern England.* (Cambridge Studies in English Legal History.) New York: Cambridge University Press. 1986. Pp. xiii, 396. $49.50.

Most work on the history of the English legal profession has focused on the men who occupied its upper echelons, such as the barristers, sergeants at law, and judges. C. W. Brooks's splendid book, however, looks at the attorneys, solicitors, and minor legal officials who ranked "below the bar" and who were concerned with the more practical, less theoretical aspects of the law. This lower branch of the legal profession was much larger than its more prestigious counterpart, and the professional lives of its members are more difficult to reconstruct. Using a carefully selected three-county sample, however, Brooks is able to present a remarkably thorough and illuminating study of these men's training at the Inns of Chancery, the conduct of their private practice both in London and in the provinces, the fees they received, and their involvement in politics and public life. His study, moreover, is set at a time when the character of the lower branch was being completely transformed and when the divisions between it and the upper branch were becoming crystallized.

The most important development with which Brooks deals, and the one that should be of greatest interest to general historians of this period, is the astonishing increase in litigation in the central courts during the sixteenth and seventeenth centuries, especially after 1560. This development Brooks attributes neither to procedural innovation nor to greater respect for law and order (the most common explanation) but to a combination of social and economic change and the growing appeal of central as opposed to local borough or manorial courts. The bulk of the new legal business consisted of actions of debt that were brought by yeomen, merchants, and other nongentry plaintiffs who found justice in the central courts not only useful in their commercial transactions but also relatively inexpensive.

The increase in litigation was primarily responsible for the dramatic growth in the number of lawyers who worked in the central courts. Between 1560 and 1640 the number of attorneys in King's Bench and Common Pleas rose from about two hundred fifty to approximately seventeen hundred

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fifty. At the same time the number of purely provincial practitioners, including amateur court keepers, declined. The increase in litigation, therefore, made the lower branch more centralized, professionalized, and cohesive, but it also led to the sale of offices, created conflict among officials, and "ultimately helped to discredit royal justice in general" (p. 121). The number of lawyers also eventually caught up with the expanded volume of casework, so that by 1640 a career was not as attractive as it had been in 1560. Some of the most interesting material in this book deals with professional income. Although a few men profited greatly from legal practice, their numbers were small indeed, and a majority of the members of the profession never owned land worth more than fifty pounds. Most of these men, therefore, never really became members of the landed class, even though they were entitled to bear the rank of gentleman. As a group they gave little evidence of upward social mobility, tending to remain within the "broad middle rank" of English society from which they had come. In this and in many other respects, the members of the lower branch differed from those men who had been called to the bar. By exploring these differences and by illustrating the effects of the momentous legal changes of this period, Brooks has greatly advanced our knowledge of the English legal profession.

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CHRISTOPHER HILL. *The Collected Essays of Christopher Hill.* Volume 3, *People and Ideas in Seventeenth-Century England.* Amherst: University of Massachusetts Press. 1986. Pp. xi, 340. $27.50.

Spending the spring semester 1987 on sabbatical in Britain, I had the opportunity to go searching in secondhand bookshops for the out-of-print volumes I needed—or just wanted—for my library. On one occasion in London, at a shop specializing in socialist studies, I came across K. E. Holme's *The Two Commonwealths* (1945) for a mere two pounds. On paying for the book, I asked the proprietor if he knew who K. E. Holme was. "No," he replied, so I proceeded to explain to him that it was Christopher Hill's pseudonym (K. E. stands for Christopher Edward, and Holme means hill in Russian). The book was written in support of Anglo-Soviet friendship during the war under the auspices of the Foreign Office. The shopkeeper was most surprised and quite sorry he had not known for, as he said, he might then have sold it at a higher price as a "collector's item," it being a "unique" text by one of England's great historians.

Hill has been writing history for about fifty years now, significantly revising our understanding of seventeenth-century England and its revolution; indeed, no scholar can now approach the period without confronting or contending with Hill's work. His major project has been to bring about the recognition that the struggles and upheavals of the seventeenth century were not just a civil war but a social revolution in the fullest sense on the historic scale of the French and Russian revolutions. This has entailed the exploration and elaboration of two theses in particular. First, the revolution that succeeded was a "bourgeois revolution" with dramatic consequences for the development of English capitalism and for the Industrial Revolution a century later. Second, within the revolution that succeeded there was another struggle, which, although it failed to emerge fully, was a primary determinant of the course of the former and provided a legacy of ideals and aspirations for later generations; this was the "democratic revolution" of Levellers, Diggers, and other radical religious groups (which, arguably, contributed a great deal to the American revolution). Naturally, these two theses have undergone revision in the course of the past five decades, but what has remained a central characteristic of Hill's scholarship—one that he shares with his former comrades of the Communist Party Historians' Group of 1946–56 (Rodney Hilton, Eric Hobsbawm, George Rudé, and E. P. Thompson, among others)—is his class-struggle analysis of the historical process and his perspective "from the bottom up."

Between 1940 and 1984 Hill authored or edited twenty books. Additionally, we now have three volumes of his collected essays—articles, review essays, and lectures not previously published in one of his own books. The first volume is primarily composed of literary studies; the second of religious studies; and the third of political and cultural studies. All three books, however, demonstrate Hill's interdisciplinary perspective on the past and his concern that scholars should not isolate literature from politics, religion from politics, nor life from politics. The first volume, *Writing and Revolution*, illustrates Hill's interest in exploring seventeenth-century literary figures in their historical contexts both for the sake of better understanding their language and ideas and to reveal more about the society in which they wrote. (This would seem to be an extension of his labors presented in *Milton and the English Revolution* [1977] but actually continues work on the questions that originally intrigued him in the 1930s when he encountered T. S. Eliot's assertion that "in the seventeenth century a dissociation of sensibility set in from which we have never recovered.") The collection opens with two extremely well written essays—"The Pre-Revolutionary Decades" and "Censorship and English Literature"—which are essential reading not only as introductions to the chapters that follow but also for those of us interested in how to

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read seventeenth-century literature as historians. A dozen essays follow, attending to such figures as John Milton, Andrew Marvell, Francis Quarles, Edward Benlowes, Samuel Pepys, and Samuel Butler. The essay I most enjoyed reading was "Daniel Defoe and Robinson Crusoe," which of course finds Hill moving into the eighteenth century.

The present volume is less focused than the first but is in some ways richer (at least for me) because it brings together a variety of Hill's articles that have been available elsewhere but in places that scholars in this age of overspecialization might not have come across. Especially, I have in mind his essays "The Poor and the People," "Science and Magic," and "Covenant Theology and the Concept of 'A Public Person,'" which were in collections honoring Rudé, Hobsbawm, and C. B. Macpherson. There are also several short pieces in which Hill writes in historical terms. For those of us who are curious as to what leading historians think of each other's efforts, I call attention to his review of Fernand Braudel. It contains no surprises, but it does highlight the difference between the Annales historians and the British Marxist historians in that Hill criticizes Braudel's marginalization of politics and the political and his inadequate concept of capitalism. Included as well is Hill's article "Parliament and People in 17th-Century England" (1981) in which he writes confidently of the demise of the "revisionist" school and sees a new synthesis emerging exemplified by the recent work of David Underdown (although it should be noted that yet a new revisionism seems to be coming over the horizon). There are two other articles that make this volume worth having. The first is "A Bourgeois Revolution?" (1980) in which Hill restates in somewhat revised fashion his primary thesis that the English revolution was essential to the subsequent development of capitalism. The second, "Radical Pirates?" I find particularly fascinating, although I can readily imagine many historians finding it too speculative or overly concerned with the margins of "civilization." Here Hill pursues the question of what happened to the radicals and their ideas in the late seventeenth and eighteenth centuries. In this case he finds evidence of their having taken to the high seas. Selfishly, this is the kind of work I would like to see Hill pursue further, but it would seem that literary studies have recaptured his imagination. Hill's collected essays make good reading. Some paperback arrangement for these is in order, perhaps a selection of the best essays into a single book; and, since he continues to publish articles, we can look forward to a fourth volume (perhaps to include his classic piece, "The Norman Yoke").

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K. H. D. HALEY. *An English Diplomat in the Low Countries: Sir William Temple and John De Witt, 1665–1672.* New York: Clarendon Press of Oxford University Press. 1986. Pp. xiii, 334. $49.00.

The government of Restoration England was an amateurish affair. There was not, to begin with, an adequate supply of men with the necessary languages to form a professional diplomatic corps. Lord Arlington frequently neglected to write to his agents serving on the Continent and sometimes let them write their own instructions. Meanwhile, the Treasury did not have enough money to pay ambassadors for their ordinary expenses and did not care to approve their claims for extraordinaries, however justified these might be. Real power still lay with the king, who could write letters and indeed make treaties without the agreement or the knowledge of his secretaries of state.

What kind of diplomat would serve such a government? He must have independent means and a patron. He should not become too attached, either emotionally or by name, to a particular policy. And he should not write too well, if he wanted to get to the top. Government is not an academic exercise; even today, when economists and political scientists jostle sociologists in the corridors of power, professorial types do not do very well as ambassadors.

Sir William Temple, like Lord Halifax, wrote much too well ever to get to the top. We read the two of them today, as we read almost no other politicians of that generation, with real pleasure. But Temple made other mistakes. He did not keep his eye on the king. In formulating his own personal foreign policy, he went far beyond his instructions, to say nothing of the king's wishes. And having concluded the Triple Alliance he took more of the credit for himself than was wise, so that it was easy for Charles and Arlington to let him go when they went over to the French alliance. Temple paid so little attention to his own court that he was not aware of the change until much too late in the day.

K. H. D. Haley does not, perhaps, entirely succeed in defending his hero from the criticisms of Lord Macaulay. Temple did not have a successful career, and his ideas, though charmingly expressed, were often neither very practical nor entirely first-rate. Although Haley himself writes very well, there will also be questions about his differences with H. H. Rowen and other scholars in interpreting an important decade in Dutch and English history. He seems to have relied on Nicolaas Japikse rather than using the original manuscripts in the Koninklijk Huisarchief, he does not refer to Fockema Andreae's criticisms of Temple, and, when he gets into the field of political thought, he does not seem to remember his Bodin. Students will, however,

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appreciate the coherent and succinct narrative, and they can learn much from Haley's quiet asides.

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IAN K. STEELE. *The English Atlantic, 1675–1740: An Exploration of Communication and Community*. New York: Oxford University Press. 1986. Pp. xii, 400. $34.00.

This is a Braudelian sort of book. The "English Atlantic" is seen, like the sixteenth-century Mediterranean, as a system of communications whose patterns and rhythms were chiefly determined by winds and currents, some of them subject to seasonal variation. The book has three parts. The first describes shipping routes and transit times. The second concerns modes of communication other than cargo shipping: postal services and newspapers on land, packet boats and naval courier vessels on the seas. (It is a pity that these two parts were not culled of repetition by skillful editing, but even if that had been done the material would not suit everyone's taste.) The third part deals with implications of the transformation disclosed by the first two. All in all, the book represents a struggle against a common misperception: "Maritime enterprises," Ian K. Steele notes, "have come to be regarded as unnatural creations whereas continental empires are seen as fulfilling natural destinies . . . The assumption that land united and water divided is dubious for early modern times" (p. viii).

Whereas Fernand Braudel's approach to the sixteenth-century Mediterranean tended to minimize the significance of change, Steele's aim is exactly opposite. His research convincingly demonstrates a "consolidation of the English Atlantic" between the 1670s and 1740, a development caused and measured by enhanced flows of information.

Communication of information is the central theme and is posited as the main driving force behind other improvements. Shipping efficiency offers an important instance. Steele relegates sailing technology to a minor role. This is a treacherous subject because evidence concerning sailing qualities is so inherently uncertain and ill-preserved, but his case is persuasive. After all, compared to the Mediterranean, which was navigated from ancient times, the seventeenth-century Atlantic was little known. Especially in the years after 1670, progress along the navigational learning curve was reinforced by published guides and also by aids to pilotage (erected mostly on the dangerous lee shores of the British Isles and the Channel). Moreover, improved transmission of commercial information reduced the time of lingering in port, and improved levels of safety from piratical capture reduced delays and costs. Regarding the last point, one could wish that among the many tabulations in the appendix there had been one that traced the declining level of peacetime losses to piracy. Although there is fairly good indirect evidence of the decline (in the North Atlantic and Caribbean after 1715 or 1720) in the form of British naval intervention, removal of deck guns from merchant ships, and lower insurance rates, a tabulation of direct evidence would have been appropriate in a book of this kind.

Communication was facilitated immensely within the "English Atlantic" by a common language, harmonized institutions, a common cause in time of hostilities, and a set of ancestral inheritances that were best nourished by links to London. (Steele's exposition of the last point—concerning social and cultural affinities—appears to me as convincing as it is likely to be controversial.) These are the factors that led the author to begin his story of consolidation in the 1670s, when the English government made its first sustained effort to assume control of decisions for organized violence on the other side of the Atlantic. Thereafter, integration occurred in war as much as peace, though in different ways and for different reasons.

Commerce presents the most fascinating aspect of the process in peacetime. It seems clear that the "English Atlantic" fostered a high level of competition within its protected and regulated sphere; moreover, it was deliberately opened or left open to external trading opportunities (at least down to 1740). In his grand perception of *histoire mondiale* Braudel was gripped by the idea that capitalism's transoceanic leaps (in this very period) were essentially "antimarket" in character. The present book does not support this idea at all.

Steele brings the history of consolidation to an end in 1740 "to avoid preoccupation with the countervailing trend that later dominated" (p. 273). His purpose, to show that water does not necessarily divide and that as late as 1740 political integration rather than disintegration was still the trend, is thus served. Matters imperial could have gone either way. Yet one is bound to ask: Were there no important seeds of disintegration being sown in the decades before 1740? The lure, cultural as well as economic, of the frontier is not addressed. Was it trivial? Was there a change during this period in the proportion of colonial population living within easy reach of tidewater? No statistical estimates of this are given. Did commercial externalities gain or lose importance down to 1740? Sailings from North American ports to the "foreign West Indies" are tabulated in the appendix but not analyzed in the text. Quite possibly, all these and other similar points might have been placed in a perspective that would have actually strengthened the author's line of argument, but they are not discussed. However

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that may be, the book is a distinguished achievement. Our conception of the foundations of England's Atlantic empire has been extensively and permanently remodeled.

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Cornell University

C. H. LEE. *The British Economy since 1700: A Macroeconomic Perspective*. New York: Cambridge University Press. 1986. Pp. ix, 297. Cloth $49.50, paper $16.95.

Britain's economic performance, both past and present, continues to fascinate economic historians and economists alike. C. H. Lee believes that better "explanations of the current malaise" can be found in Britain's distant economic past than in its more recent history (p. 3). Using 1914 as the watershed, he divides British economic history since 1700 into two periods and develops parallel chapters that lay out the conventional framework of macroeconomic analysis: aggregate demand, investment and the financial system, production and productivity, aggregate supply and economic growth, foreign trade and payments, and regional growth. There is a separate chapter on government policy and its effects on the economy in the twentieth century. Although most of the analysis and discussion is national in scope, the more intriguing observations emerge when Lee strays from the aggregate approach and considers regional patterns of growth, the major sectors of the economy, and even individual industries and firms. He concludes, for example, that the regions that enjoyed the most rapid industrial growth in 1700–1914, namely the north and west, nevertheless grew more slowly in terms of incomes than the other regions. Despite government social policies, enormous differences in regional incomes have emerged since 1950, with the rapid decline of industry in the northeast, Scotland, and Wales.

Lee's conclusions are seldom earth shattering: there was no great discontinuity in economic growth in the late eighteenth century; growth rates were consistently lower than those achieved by other industrializing nations; foreign trade was vital to the British economy in the nineteenth century; inequality in the distribution of wealth and income has been reduced in the twentieth century; and government intervention in the economy since World War II has focused on the short-term problems of inflation and unemployment to the exclusion of longer-term growth objectives.

He argues that the peculiar structure of the British economy and the ways in which growth occurred from the eighteenth century on made the relatively poor economic performance of the late Victorian era and beyond inevitable. Small, family-owned, specialized manufacturing firms, using labor-intensive methods, with few connections to the financial sector, produced industrialization and steady but slow growth in the late eighteenth and early nineteenth centuries. At the same time, Britain attained a dominant position in international trade and finance, but this had no significant positive influence on the rest of the economy.

From about 1850 on, and especially after World War I, the British economy fared poorly compared to all other advanced economies of the West in terms of overall growth, productivity, and the standard of living. There is one striking indicator of decline: Britain's share of world industrial production fell from nearly 23 percent in 1880 to 4 percent by 1980. Lee argues that the late Victorian economy performed as well as it could have, given the dominance of the small family firm and other inherited institutional rigidities. Even the winds of international competition, which increased in velocity by the late nineteenth century, failed to generate the needed investments in new, large-scale plants using innovative technology. British firms instead responded to competitive pressures by cutting wages and employment.

The macroeconomic framework that Lee uses throughout this volume is generally not a useful approach to the more interesting questions that he is trying to address, particularly the causes of Britain's economic decline in the Victorian era and beyond. This is a valuable addition to the literature in that it provides a summary and synthesis of recent work on British economic growth, but it does not cut any new ground.

CHARLES K. HYDE
Wayne State University

SYBIL WOLFRAM. *In-Laws and Outlaws: Kinship and Marriage in England*. New York: St. Martin's. 1987. Pp. 253. $35.00.

In the first half of this book Sybil Wolfram describes the English kinship system as it traditionally existed and the changes that have taken place in it in the nineteenth and twentieth centuries. Besides outlining the history of divorce and married women's property, subjects generally well known, she surveys subjects that are unfamiliar. She lays out the ecclesiastical prohibitions on marriage between relatives and shows how these have been relaxed. She describes the customs that governed mourning and discusses English kinship terminology.

Wolfram finds the key to the English kinship system to lie in the concept of the unity of husband and wife, a concept now much eroded. This concept she sees as having made for the prohibitions on

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adultery and divorce, for the law on married women's property, and for the prohibition on marriage with affines, that is, with relatives by marriage. In the light of this concept she manages to make significant that marathon wrangle over the deceased wife's sister bill, now usually a subject for merriment.

Wolfram is a philosopher, and in the second part of her book she sets out to show that common beliefs about kinship are wrong. Principally, she questions the common belief that the function of the rule prohibiting marriage between relatives is to prevent the birth of defective children. She finds the belief to be scientifically ill-founded, Robert Bakewell having proved that inbreeding need not be harmful: it merely intensifies the qualities, good or bad, that are in the stock. She then considers functional explanations that anthropologists have suggested for the rule, finding all of them wrong too. She ends with what is a sweeping theory about kinship rules generally. The rule prohibiting marriage between relatives serves "no more tangible a purpose than marking off relatives from non-relatives . . . Why do we suppose that customs . . . that appear to mark categories have any particular purpose beyond just that of marking categories?" (pp. 204–05).

Unfortunately for this theory, the rule prohibiting marriage between relatives, between at least close relatives, does have the function commonly ascribed to it. This is the conclusion of E. O. Wilson from genetic calculations. Wilson, moreover, points to evidence demonstrating just how serious are the ill-effects of close inbreeding in humans, and he describes habits that in effect limit inbreeding in animal societies. Bakewell's work easily misleads. Close inbreeding has improved domestic animals for our purposes. They, however, have been rendered less fit than their wild kin.

At some length Wolfram attempts to prove that another belief about kinship is wrong. Society is wrong, she declares, to believe that the rise in divorce is due to the emancipation of women. Now society does not believe that the rise in divorce is due solely to the emancipation of women; it certainly holds that the decline of the church, which affects both men and women, has had a great deal to do with the rise in divorce and probably even holds this to be the overriding cause of it. Wolfram mentions the existence of what she calls "subsidiary hypotheses" (p. 149), but she never specifies them. She hammers away as if society believed that the emancipation of women was the sole cause and ends by pointing to the decline of the church.

This book provides a brief survey of English kinship rules, especially of the rules on incest and prohibited degrees, but its ambitious attempt to prove that social beliefs on kinship are wrong is misguided.

EILEEN SPRING
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TERRY CASTLE. *Masquerade and Civilization: The Carnivalesque in Eighteenth-Century English Culture and Fiction*. Stanford: Stanford University Press. 1986. Pp. x, 395. $37.50.

Terry Castle has given us an analysis of the English masquerade, as both cultural institution and fictional topos, that is as insightful as it is thorough. The first two chapters treat the public masquerade as the immensely popular form of urban entertainment it was in eighteenth-century England. Castle bases her discussion on a wealth of contemporary accounts. She examines possible historical precedents and similar Continental customs, and she presents a detailed picture of the peculiarly English phenomenon.

In function the English masquerade had much in common with carnivals, fairs, and masked balls throughout Europe. It was a time and place where social class boundaries were transgressed, where sexual liberties were permitted, where all the norms of polite society were suspended for the space of those few hours. Castle discusses the mechanisms of this inversion: costume, the use of masks, the "squeaking" voice adopted by participants, women masquerading as men and vice versa.

After painting a vivid picture of this swirling, topsy-turvy world, Castle tackles the question of its demise. As cultural institution and fictional device, the masquerade in England was dead by the end of the eighteenth century. Castle uses the compelling analyses of Mikhail Bakhtin and Roger Caillois, which posit the ascendency of the world of rationalism with its bourgeois decorum and passion for neat categorization and the corresponding death of the carnivalesque celebration of transformation and mimicry. Her own important contribution lies in the analysis of the antimasquerade rhetoric of the time.

The remaining chapters of Castle's book take up the masquerade theme in the fiction of four eighteenth-century English writers. Just as the masquerade as institution dealt in transformations, so, Castle argues, did it function in novels as a point for narrative transformation, though not in identical ways for each writer. Samuel Richardson's *Pamela*, the earliest of the novels, is presented as a kind of test case of the use of the masquerade as a plot catalyst. Henry Fielding's *Amelia* illustrates its author's ambivalence toward the paradoxical nature of the mask with its potential for both deceit and liberation.

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The chapters on Fanny Burney and Elizabeth Inchbald are a real tour de force treating the feminist and utopian strains in women's writing. Castle has previously argued for the masquerade as an institution that allowed women unheard-of freedoms, an institution where all is possible and all is risked. In Burney's *Cecilia*, the heroine meets her future husband at a masquerade, but, instead of bringing about an elevation of her fortunes, it causes the reverse. Cecilia is forced to give away her inheritance and independence as the price of marriage: "Burney's novel is a dystopia, the projection of failed revolutionary hopes" (p. 289).

The same is not true for Inchbald's *A Simple Story*. Its heroine, Miss Milner, tests the extent of Dorriforth's love for her by going to a masquerade against his orders. Having risked all, she wins all. In this book the masquerade is simply a device for the test of wills. In this use and in the outcome, Inchbald has progressed far indeed from Richardson.

The final chapter takes up the fate of the masquerade theme after the eighteenth century. Castle suggests that the impulses given voice in the masquerade have increasingly become internalized so that the transformations of situations and characters are handled by the portrayal of individual psychological states. It is a shift from a concern with the representation of a communal ethos to the fate of the individual.

Castle's book is a delight for the eyes as well as for the intellect. There are forty illustrations, some of which are old friends but many of which are not familiar. The end papers reproduce Charles White's rollicking *Masquerade Scene at the Pantheon*. Each chapter opens with an engaging print from Dover's *Catchpenny Prints*. In short it is a lovingly produced book.

It is also clearly a lovingly written book. Castle skillfully moves back and forth between cultural and literary analysis. In the process she presents fresh and penetrating insights that invite us to think of related phenomena, for example, French pantomime and Italian commedia dell'arte. The reader is caught up in the force of the argument and captivated by the wit and elegance of the writing. It is an ambitious book that more than fulfills its promise.

ANYA PETERSON ROYCE
Indiana University

N. A. M. RODGER. *The Wooden World: An Anatomy of the Georgian Navy*. Annapolis, Md.: Naval Institute. 1986. Pp. 445. $21.95.

"Don't talk to me about naval tradition," Winston Churchill was quoted as saying, "it's nothing but rum, sodomy and the lash." Such a characterization, if N. A. M. Rodger is correct, was seriously misleading, at least of the Royal Navy at the time of the Seven Years' War (1756–63). Men drank beer and, when it ran out, watered wine and as a last resort watered spirits: rum in the West Indies, arrack in the East Indies, and brandy elsewhere (p. 73). Homosexuality was rare, with only eleven courts-martial in the period under study and but seven convictions. Seamen were "young single men of vigorously heterosexual inclination" (p. 81). Certainly, the lash was used, and heavily when theft was involved, but there were few sadists among the officers, and their brutality was not tolerated once it became known. Flogging was a frequent enough punishment on land as at sea, but its indiscriminate use brought charges of cruelty against an officer. "Insubordination in every form and from every rank and rating in the service was a daily part of life. Where modern officers expect to command [in the Royal Navy], mid-eighteenth-century officers hoped to persuade" (p. 206).

With such arguments, based on a wide reading of the abundant Admiralty papers in the Public Record Office and the National Maritime Museum, Rodger attempts to transform our idea of the British navy two centuries ago. In its simplest form his thesis demonstrates that the navy was neither as harsh nor as socially divided as has usually been thought. Whatever the navy became in Horatio Nelson's day and in the nineteenth century, the navy in the era of the Seven Years' War and American revolution was certainly not a "sort of floating concentration camp" (p. 344). Rodger's navy "resembled the society from which it was recruited in many more ways than it differed from it" (p. 345). Seamen possessed a rare skill, which both they and their officers fully understood. The mere use of force would have precluded great successes at sea. He argues that the fleet that mutinied in 1797 must have endured very different conditions from those that obtained forty years earlier. For the officers seamen were "like forward children, not knowing how to judge for themselves, but . . . to be cherished as the first born of a fond parent" (p. 118).

Sea life was certainly hard. On average, warships spent more than 40 percent of their commission time at sea. Yet the larger the ship, the easier the seaman's life and the greater his comfort. Danger came not from combat but from uncertain navigation and the weather. Disease was the principal killer. Yet with a good diet, exceptionally clean ships, and enough exercise and fresh air, seamen were far healthier than the populations ashore in the port towns from which they sailed. Still, seamen deserted in large numbers. Overwhelmingly, such men had been pressed into service. They escaped whenever opportunity arose, especially in the home ports, North America, and the West Indies. Rodger shows that there was a large traffic in desertion from

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one ship to another. Especially did they desert the small, cramped, and leaky sloops for the "relative comfort and less strenuous work" of a ship of the line (p. 193). Often, such desertion was connived at by officers. That the health of seamen was important to the Admiralty is clear Rodger argues from not only the efficiency of the Victualling Office but also the erection near Gosport of the most modern hospital in Britain. It became the "prototype of a new regime in treating sick seamen, under professional care in buildings designed for the purpose" (p. 110). It also had a twelve-foot wall surrounding the complex to discourage desertion. If it was not actually a charnel house, it certainly resembled a prison hospital.

With more than two-thirds of his text devoted to seamen and the rest to their officers, Rodger has probably hit on an acceptable balance. Earlier studies have shown far greater familiarity with the world of the naval officers. Perhaps for this reason the second part of his book seems less novel. Still, the book as a whole is thoroughly refreshing, with a compelling thesis convincingly argued with a formidable array of evidence. I began the first chapters rather skeptical of some of his conclusions. I ended by having to recast my own thinking about a body of men I had thought I knew well.

JULIAN GWYN
University of Ottawa

WHITNEY R. D. JONES. *David Williams: The Anvil and the Hammer*. Tuscaloosa: University of Alabama Press. 1986. Pp. xviii, 266. $29.95.

David Williams (1738–1816) was an Enlightenment figure of many roles, and his life serves to remind us of the remarkable enthusiasms unleashed in that age. Protracted into a time of enthusiasms spent, the life equally reminds us that the world may not cherish original minds housed in unclubbable persons.

This is the first book-length scholarly account of Williams, and a very satisfactory one it is. Whitney R. D. Jones has had to master a large body of materials, for Williams was not an obscure person. Possibly undervalued by contemporaries, clearly disliked by many, Williams left a considerable record in books and manuscripts. Among the matters dealt with here is Williams's religious odyssey from conventional nonconformity to the further reaches of deism and Unitarianism. This account clarifies a lot about Williams but not the question that occurred to this admirer of his thought: "Are there dissenters other than Protestant Dissenters, and if not, to what general category did people such as he belong?" A career as teacher and theorist on education, the founding of the Royal Literary Fund—his obvious monument—and diplomatic ventures, both for republican France and for Britain, all have their place here. But the largest part of the story is concerned with the development of an original, often obscure, political theory.

The image of hammer and anvil was suggested by Benjamin Franklin's comment about the scheme for a literary fund, when he said that Williams, the hammer, might be worn out by the anvil of the status quo. But, as Jones says, it serves best to chart the progress of Williams as political theorist (p. 158). A radical, reconciled to universal male suffrage by his plan of a pyramid of representation based on small units, Williams could never find common cause with any large movement. Powerfully aware of the dangers of mass politics or of any rapid change, Williams was prone to proclaim the faults of British government in the strongest terms, while offering no ready avenue of improvement. After his experiences in revolutionary France, Williams became yet more cautious about advocating reform. This treatment contributes some compelling new evidence of the degree to which Williams was reconciled, in later life, to British institutions, although Jones is careful to insist on the continuities in his political thought.

Among the insights to be found in Jones's analysis is the suggestion that Rousseau may well have been an inspiration for Williams's social organicism, culminating in the effort to locate and develop a "sensorium" for society similar to the human nervous system. Jones also offers the happy thought that Williams seems to have sought to make society a sort of person to compensate for the element of anthropomorphism that he had ruled out of his religious faith (p. 178). Less convincing is the recurrent effort to liken Williams's organism to Tudor categories, for this surely tells us more about our author's previous work on the sixteenth century than about Williams in the eighteenth. Throughout, Jones shows sound judgment in never claiming too much for the originality or influence of his subject. Possibly he could even have been more sympathetic to Williams's efforts at inventing a vocabulary for the understanding of public opinion.

It is hard to quarrel much with the painstaking research and judicious use of evidence found here, but two minor objections may be warranted. One must resist the notion that Williams could ever have agreed with the categories of John Reeves, the high Tory (p. 140). Second, consultation of the article on Williams in the *Biographical Dictionary of Modern British Radicals* would have told Jones that the manuscript by Williams in the Pelham papers, British Library, was already known.

This is a pleasantly written book, well argued and without pretension. If anything, claims are understated. One gathers that Williams is attracting other

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substantial studies. Given the manifest strengths of this book, one wonders if their services will be needed.

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BIANCAMARIA FONTANA. *Rethinking the Politics of Commercial Society: The* Edinburgh Review, *1802–1832*. New York: Cambridge University Press. 1985. Pp. viii, 256. $34.50.

This is a book that needed to be written. Two things have long been known. First, the *Edinburgh Review*, founded in 1802 and destined to become the most influential British periodical of the early nineteenth century, was steeped in the historical thought of the Scottish Enlightenment, in the "conjectural history" to be found in the works of (among others) David Hume, John Millar, Adam Smith, and perhaps above all in the writings and lectures of Dugald Stewart, who conveyed the essence of those works to his student audiences at the University of Edinburgh in the years immediately preceding the *Review*'s founding. Second, the *Review* played a significant part in the advocacy of British parliamentary reform, and any discussion of public opinion favorable to the Reform Act of 1832 must include some mention of the *Review*'s influence.

But, thus far, no one has tackled the difficult problem of the relationship between Enlightenment thought and political reform, part of the more general question of how the *Review*'s eighteenth-century intellectual heritage was adapted by its chief contributors to the needs of those readers—some Scottish, an increasing number English—who required a popular version of political reformist thought that took account of the French revolution, the Napoleonic wars, and the social and economic strains and dislocations undergone by British society in the 1820s. Biancamaria Fontana has tackled that problem, and, although her book is far from easy reading, she has produced an illuminating and subtle monograph that should interest not merely students of early nineteenth-century British thought and opinion but all historians of ideas concerned with the ways in which, at times, a congeries of ideas from a previous generation may be usefully applied to the needs and preoccupations of its successors.

A key concept of Scottish conjectural history, the growth of commercial out of feudal society, a growth marked by the increasing number and power of "the middling ranks" who helped bring about economic progress in the form of material improvement, became the chief device employed by the Whig *Edinburgh Review* to take account of the momentous events marking the social and political history of Europe during the first third of the nineteenth century. After all, the emergence of the middling ranks could be said to have been the essential feature of the French revolution. That revolution (so Francis Jeffrey, the *Review*'s editor, wrote) had profoundly affected Enlish political life by the threat of armed conflict between "democrats" and "courtiers," a conflict capable of destroying all the benefits of commercial progress.

Political parties—here Edmund Burke proved as important for the reviewers as Millar and Stewart—were no longer parliamentary factions but parties in the country, divisions within society and public opinion. The question for the *Review* became what role the Whig party should now play, not just in Burkean constitutional terms of preserving liberty and order but also—and here the sociological emphasis of eighteenth-century Scottish thought reasserted itself—in terms of which social groups and forces in society it should attempt to represent. In other words, the theory of the growth of commercial society as an outstanding feature of modernity had by no means lost its explanatory power in the reviewers' continuous effort to develop a science of politics in the tradition of their eighteenth-century predecessors. In the generation before the first Reform Act, the phrase "middling ranks," not clearly defined in the eighteenth century, attained in the pages of the *Review* a more specific sociological content.

The reviewers could not be said to have constituted a politically unified group in agreement about detailed proposals for economic or other reforms. One of the great virtues of this book is the author's firm refusal to talk about neat categories where none existed or to make claims for specific agreements when those were absent. But, although there were certainly differences among the reviewers—their opinions about reform were "heterogeneous and not altogether coherent" (p. 149)—they shared a faith in the political and human resources of modern commercial society to resolve the main difficulties it faced. They agreed, furthermore, that a science of politics was desirable and possible. Finally, they managed to combine a strong empirical sense of day-to-day political reality with the theoretical heritage that had helped shape their thought.

JOHN CLIVE
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NEVILLE THOMPSON. *Wellington after Waterloo*. New York: Methuen or Routledge and Kegan Paul, London. 1986. Pp. xiii, 303. $45.00.

This is the best twentieth-century account of the personality, private life, and political career, after 1818, of Arthur Wellesley. Neville Thompson's

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duke is convincing: a reflective man, even Catonian, practical to the point of asceticism in matters of personal discipline, yet annoying, vain, and full of apocalyptic fears and demons. The author seems right on the mark in displaying a Wellington who was "one of the great conversationalists of all time" (p. 23), indeed, the peer in that regard of Samuel Johnson himself. The most polished chapter in the book is the first, in which Thompson excels in describing the duke's emotionally fraught relations with his cold mother, his eccentric brothers, his unloved wife, and his disappointing children.

The author also convinces in his quite skillful integration of the personal qualities of the duke with his political stances. Wellington, after all, was a more paradoxical politician than most of the genre. The "Protestant" who led the successful struggle for Catholic Emancipation, the firm Tory opponent of parliamentary reform who was ultimately prepared to pass nearly as radical a bill as his Whig opponents, the Anglo-Irish aristocrat who favored a heavy tax on absentee Irish landlords, and the agricultural protectionist who persuaded his order to allow the repeal of the Corn Laws is explained by Thompson as an individual usually prepared to place the cause of firm and stable government before more ephemeral ideological or party issues. Hence, his career prefigured Charles de Gaulle's in the twentieth century as much as it mimicked George Washington's in the eighteenth.

The author's sections on Wellington's career from 1818 to 1832 contain few surprises. The pre-1832 years of Wellington's life were the subject of a massive act of Victorian filial piety by the second duke, who edited a magisterial compilation of twenty-three volumes of his father's dispatches, correspondence, and memoranda. Few, if any, scholars, however, have made such good use of the duke's unpublished political correspondence of 1832 to 1852. Thompson greatly enlarges the current state of our knowledge of Wellington's crucial role as Tory leader in the Lords, especially in 1846.

The only disappointing aspects of the book are the numerous errors on nonducal matters. A distinguished historian of the 1930s, Thompson is evidently not yet fully at home in the interstices of early nineteenth-century politics. Grenvilles are confused with Granvilles (p. 137). The first and second earls Fortescue are mixed up (p. 177). Charles Wynn, a pro-Catholic Canningite-of-sorts, who ended his long parliamentary career as a Peelite, is declared an Ultra-Tory (p. 151). The Marquis Wellesley in 1821 is termed a Grenvillite (p. 37). Sir John Conroy is proclaimed the "lover" of the duchess of Kent (p. 179). More serious is Thompson's tendency to rely too heavily on the trustworthiness of his sources. Hence, on the authority of Princess Lieven of all people, he repeats as fact the preposterous story of the Princess of Wales receiving a letter of freedom from marital constraints from her husband and engaging George Canning in sexual congress "on the spot" (p. 36).

JAMES J. SACK
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RONALD K. HUCH and PAUL R. ZIEGLER. *Joseph Hume: The People's M.P.* (Memoirs of the American Philosophical Society, number 163.) Philadelphia: American Philosophical Society. 1985. Pp. viii, 172.

Ronald K. Huch and Paul R. Ziegler have investigated the fascinating era in which capitalism was born by means of a fine and much needed biography of Joseph Hume (1777–1855), one of Britain's most important "radical" politicians. Much can be learned by tracing the changing meanings of radicalism and capitalism over the past two hundred years. When capitalists were radicals, radicalism stood for peace, retrenchment, and reform. It aimed to eliminate the special privileges of the landed aristocracy and the established church, to enlarge the size of the electorate, to establish a national system of secular education, to make government more efficient, and to end state regulation of the economy so that all individuals might freely compete and thereby release, in accord with the principles of the classical economists, the productive potential of the nation.

It is unfortunate that Hume's private papers were destroyed; one would like to know more of his personal life. Born to a lower-middle-class family in Scotland, he still managed to study medicine at the University of Edinburgh. In 1797, seeking fame and fortune, he joined the East India Company as a physician. Within a few years, having mastered Hindustani, he drifted away from medicine and accepted responsibility for feeding and clothing a part of the British army in India. By 1808 this bright, clever, aggressive young man had acquired a fortune of forty thousand pounds and prepared for a triumphant return home. If he could achieve so much on his own initiative, why not everyone else?

Mistakenly regarding himself as a Tory, Hume purchased a seat in the House of Commons in 1812 but quickly antagonized his patron and brought his first term in Parliament to an abrupt end when he opposed legislation designed to stabilize wages and prices in the framework knitting industry. Hume took the position that characterized the middle-class, free-trade, laissez-faire radicals when he argued against regulation of the economy and urged instead that uncontrolled competition would be beneficial to both capitalists and artisans. While out of Parliament between 1812 and 1818, he solidified his

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ties with both middle-class and working-class radicals. His links with the working-class reformers were shaped through his friendship with Francis Place, the well-known tailor of Charing Cross Road. Hume and Place were usually in agreement, but Hume, returning to Parliament in 1818 and serving almost continuously until his death, was always much more prepared to compromise with the Whigs in order to keep them in office.

Speaking constantly and often forcing divisions, Hume became a bore to his opponents, but he turned into a subtle and effective committee member. He is probably best remembered for winning repeal of the Combination Acts in 1824 even though he did not approve of trade unions. It was simply wrong for the state to deny workers the right to organize. He never tired in his advocacy of shorter Parliaments, the secret ballot, and the extension of the suffrage. As death approached, Huch and Ziegler note that Hume opposed Britain's entry into the Crimean War. Why, he asked, did Britain's rulers rush to help those supposedly fighting for freedom abroad when they feared democracy at home?

Given the comparative smallness of the capitalist enterprises that Hume observed, it is not surprising that he emphasized the advantages of unchecked competition. He did not perceive that competition could lead to monopoly and gross inequities in the distribution of wealth and income. Hume struggled valiantly to achieve a liberal society; he did not foresee that one of the major unresolved problems of the twentieth century would be how to build a new meaning for the freedom he cherished so that it would mean liberty for all—or equality—rather than merely liberty for the few.

THOMAS J. SPINNER, JR.
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NICHOLAS C. EDSALL. *Richard Cobden: Independent Radical.* Cambridge: Harvard University Press. 1986. Pp. xiv, 465.

This is an outstanding biography; it is eloquently written, intelligently organized, and admirably researched. Nicholas C. Edsall's book will stand among the very best of numerous recent works on the lives of the free-trade radicals. Perhaps the most impressive aspect of this biography is that Edsall never allows his compassion for Cobden to distort his assessment of Cobden's triumphs and failures.

No one familiar with the free-trade movement will doubt the significance of Cobden's contribution to that cause. Although several scholars in the past thirty years have discussed Cobden's career from various angles, Edsall's work is the first full-scale study of the "Manchester" radical in the twentieth century. This is surprising, for the Cobden papers, particularly those in the British Library, long have been a principal source for students of nineteenth-century radical politics.

It is well-trodden ground, but anyone who writes about Cobden is required to give considerable attention to the Anti–Corn Law League. Edsall does not have any major revelations for us on this subject, but his discussion of Cobden's league is fresh, especially when he touches on Cobden's ideas of organization and his relationship with other league leaders. Edsall is correct to point out that, although many, including Sir Robert Peel, deserve recognition for Corn Law repeal in 1846, Cobden was the catalyst for this free-trade success. He thus restores the traditional view, under challenge in some quarters, that the league's agitation was the principal reason for Corn Law repeal.

For nearly fifteen years after Corn Law repeal, Cobden, like so many other free-trade radicals, seemed at loose ends. He wanted to keep the reform effort alive, but postrepeal prosperity (which Edsall controversially, but I think correctly, attributes to repeal) and the need to recover from a period of intense political activity left the reformers without a focus or a constituency. Cobden tried to help Joseph Hume revive an interest in parliamentary reform (the "Little Charter"), but the House of Commons showed only impatience. Edsall explains quite well the futility and ennui that overtook reformers after 1846. In retrospect, it is clear that the free-trade radicals were the "best and the brightest" politicians of the midcentury era, yet they could gain little following. There were some who blamed Cobden for failing to fulfill the promise of leadership, but the organizational strategy that worked so well for the Anti–Corn Law Campaign was not adaptable to the radical interests of the 1850s.

For more than a decade after Corn Law repeal, Cobden found little satisfaction in his personal or public life. Political reform, educational reform, opposition to the Crimean War (and to the policies of Palmerston in general), all efforts to which he dedicated his time, made no headway. Tragedy dogged his personal life. His young son died unexpectedly in a German boarding school, his brother died shortly thereafter, and, most discouraging of all, his wife suffered a serious mental disability for several years. Yet this was not all. Cobden experienced ruinous financial setbacks that forced him to depend on the charity of his acquaintances. In fact, it is an irony that this champion of middle-class concerns had, on many occasions throughout his life, to rely on outright gifts of money. As Edsall points out, Cobden's enemies did not fail to note his startling lack of business acumen.

Despite the problems of the late 1850s, there was one notable effort ahead for Cobden. After turning

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down a cabinet post in Palmerston's new government in 1859 because he had long-standing differences with the prime minister on foreign policy issues, Cobden received a proposal from the French political economist, Michel Chevalier. Chevalier, as he had on an earlier occasion, suggested that Cobden negotiate an agreement with France to reduce tariffs. This proposal led Cobden to the second great achievement of his life, the negotiation of the Anglo-French Commercial Treaty of 1860. Edsall, in a most valuable portion of his book, expertly takes us through the delicate discussions that Cobden carried on as a diplomat without portfolio. His fascinating, if frustrating, dealings with Napoleon III are carefully recounted. Cobden's success led to offers of pensions, a baronetcy, or a place on the Privy Council. He refused them all. Instead, during the last five years of his life, he used his new-found popularity and influence to argue for peace in the face of what he saw as Palmerston's inclination toward bellicose resolutions of all international problems.

RONALD K. HUCH
Dickinson State University

WENDY HINDE. *Richard Cobden: A Victorian Outsider*. New Haven: Yale University Press. 1987. Pp. xi, 367. $30.00.

Wendy Hinde has produced a fine and discerning new biography of Richard Cobden, based on meticulous research and written with style and grace. Appropriately in a biography, she concentrates on the man rather than the historical background. Thus, the reader will learn nothing new about the Anti–Corn Law movement or the other political campaigns in which Cobden was involved. There are no startling new revelations about Cobden himself either, but a much fuller, rounder, and more human picture of the man emerges from Hinde's pages.

Hinde traces Cobden's life from the Sussex farm where he was born and to which he later returned. In between he was a London merchant and a Lancashire manufacturer, as well as the main leader of the Anti–Corn Law League. But he was never entirely at home in the years that he was away from the county of his birth. Neither was he when he returned. His strong antiaristocratic prejudices, stronger than most of his middle-class contemporaries, made sure of that. In the words of Hinde's subtitle, Cobden was a Victorian outsider. The country of his dreams was across the Atlantic, and he once thought he would like to end his life as an Illinois farmer.

The ironies of Cobden's career are handled well by Hinde. But she also conveys the honesty and decency that were basic to his character and that made him an attractive figure. He was quick-tempered but usually equally quick to forgive. The great exception was the bitter quarrel with Sir Robert Peel that originated in a speech of Cobden's in 1843, in which he declared that the prime minister was responsible for the violence and suffering the country was undergoing. Peel took the speech itself as an incitement to violence, even a call for his own assassination. Perhaps in view of the league's questionable activities the previous year, Cobden found this accusation rather too close to the bone. The quarrel would go on until the repeal of the Corn Laws, when it dissolved in the handsome compliments the two men paid each other.

Peel, for his part, gave Cobden all the credit for the repeal of agricultural protection. That was going too far. But, on the other hand, unless one assumes that Peel could have converted his party to his own blossoming free-trade views, it is difficult to see how the Corn Laws would have been repealed at this time without the league. For Cobden it was a great personal triumph.

But, with the exception of the Cobden-Chevalier treaty in 1861, which provided another brief moment of glory, it was all downhill thereafter. After repeal Cobden concentrated his efforts on the twin causes of peace and retrenchment. Unlike the Quakers, he was not a pacifist on principle; he simply disapproved of the Crimean War in particular and the country's aggressive foreign policy in general. He got no thanks for his stand, and it temporarily lost him a seat in Parliament. Nonetheless, Cobden's prestige remained high, and in 1859 Lord Palmerston offered him a seat in the cabinet as president of the Board of Trade. Much to Palmerston's amusement, and incredulity, Cobden declined on the grounds that he could not be publicly critical of the prime minister, as he knew he would have to be, in the cabinet. Cobden's last political act before he died in 1865 was to advise that Britain break its ties with Canada in order to avoid conflict with the United States. Cobden pursued his own idiosyncratic way until the end.

RICHARD W. DAVIS
Washington University

BERNARD SEMMEL. *John Stuart Mill and the Pursuit of Virtue*. New Haven: Yale University Press. 1984. Pp. xi, 212. $17.50.

Bernard Semmel's book is an interesting discussion in a nineteenth-century context of a perennial problem: the reconciling of free will and determinism. The focal point of his discussion is John Stuart Mill's attempt, in order to escape a threatened

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lifetime of depression, to wriggle out from under his Benthamite legacy of men and women born as undifferentiated and not particularly interesting lumps of clay and find a philosophical explanation for their becoming differentiated, interesting, progressive, thinking people voluntarily choosing virtue. How molding clay could be transformed into improving humanity was a disturbing worry for Mill throughout his life.

Semmel touches on many nineteenth-century figures as he follows Mill's odyssey; the teachings of Carlyle, Comte, and the German transcendentalists receive extended treatment. A knowledge of the context of nineteenth-century polemics and a familiarity with the texts, particularly Mill's, are assumed, enabling the eclectic argument to stay within (a blessed) two hundred pages. The book is written for all nineteenth-century specialists, but especially for those who have failed to realize that Mill "sought to adopt the views of the period of German 'intellectual fermentation' as the most effective means of preserving individual freedom in the future" (p. 165).

The strength of this claim in the last chapter, on *On Liberty*, does not seem justifiable. Semmel argues that the "support German philosophy gave to liberty was the principal theme of Mill's best-known tract." He continues: "The conflict between philosophical liberty and necessity . . . was critical to the argument of *On Liberty* . . . Curiously, however, the very first sentence of the tract explicitly denied any intention of discussing this metaphysical question . . . But why begin a tract on 'civil or social liberty' with this unnecessary disavowal?"(p. 165). Any argument that dismisses the author's stated intentions in favor of the critic's insight is off to a wobbly start. Perhaps Mill preferred to argue from utilitarian premises to persuade his English contemporaries because he found the German ones hard to digest. He said of Hegel "that conversancy with him tends to deprave one's intellect . . . For some time after I had finished . . . all such words as *reflexion, development, evolution*, etc., gave me a sort of sickening feeling which I have not yet entirely got rid of."

The stress on the German transcendentalists weakens for me the force of the important point that Semmel wants to impress on his own contemporaries. In the epilogue, "Liberalism at the Crossroads," he argues that the question of individual responsibility is still the burning issue one hundred years after Mill: "In the past generation, social scientists—resembling the scientific and technical cadres of Comte's elite— . . . attributed none of man's ills to himself or to the human condition but rather regarded all as consequences of a remediable malfunctioning of society" (pp. 193–94). Mill's insistence on the pursuit of virtue is as necessary now as it ever was.

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MARTIN DOUGHTY, editor. *Building the Industrial City.* (Themes in Urban History.) Leicester: Leicester University Press; distributed by Humanities, Atlantic Highlands, N.J. 1986. Pp. xii, 212. £27.50.

The title of this volume refers to the building industry as such. Its four long essays concern working-class housing of the nineteenth century in the north of England and Scotland, and each essay adopts a particular viewpoint. Jane Springett writes of land development and the builders in and around Huddersfield: the prevalence of large estates and predictable forces of profit, snobbery, and need render the scene not very different from the better-known history of London in the period. M. H. Yeadell provides the West Riding of Yorkshire with a history of its building societies, a principal means of financing low- and middle-income housing. The earliest societies, in the last decades of the eighteenth century, were not strictly financial institutions but clubs gathered for the construction of houses on a particular block of land. They did not function as savings institutions and were disbanded when the project was complete. Societies inspired by these beginnings made possible a great deal of housing in the century that followed, though no longer mainly working class. Thomas A. Roberts doubles the interest of his account of Liverpool by attaching it to the history of the Welsh community in that city. Welsh settlers, or some of them, had ties with the suppliers of building materials in their homeland. To what degree their activities can be credited with taking Liverpool out of the cellars and courts into terraced housing remains a little unclear; Roberts does succeed in adding a narrative and communal interest to his statistics. Richard Rodger, finally, writes of a different sort of housing, the tenement, in Scotland. His essay is the most moving in the collection because in the first half he adopts essentially the point of view of medical officers of health at the end of the century. In the remainder of the essay he suggests some of the economic and legal reasons for the tenement system. All these contributions, but especially Rodger's, remind us of the stark economic differences between north and south in the United Kingdom today.

Martin Doughty's introduction to the book, although stressing that more of this kind of history needs to be done, pleads repeatedly and a little wistfully for some answer to "the problems of data comparability" (p. 7). This is ironic, for when urban history got under way about twenty years ago in

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England it was with the feeling that each northern city was unique. What is the interest of more such history? The present volume offers a wealth of minute statistics—much of it new, since the essays originated in research for theses. Yet the scene can remain intensely local: for example, "in 1865 the Leeds Medical Officer of Health thought Ebenezer Street was the filthiest street in the town" (p. 60). Only strong narrative interest, immediate relevance to the present, or compellingly argued, counterintuitive conclusions can sustain this history.

ALEXANDER WELSH
University of California, Los Angeles

DONALD M. LEWIS. *Lighten Their Darkness: The Evangelical Mission to Working-Class London, 1828–1860.* (Contributions to the Study of Religion, number 19.) Westport, Conn.: Greenwood. 1986. Pp. xiv, 369. $35.00.

Donald M. Lewis contributes to our knowledge of Victorian religion in two ways. The first, which is not evident from his title, is his reevaluation of the leadership of the Anglican evangelical party after the death of William Wilberforce in 1833. For decades historians have asked for more research on mid- and late Victorian Anglican evangelicalism, and Lewis responds with a valuable account of the founding of the influential *Record* newspaper, the spread of premillenarianism, and the advent of a greater willingness to cooperate with evangelicals of other denominations.

Lewis's second contribution takes him beyond the leadership of the evangelical party to an analysis of one particularly important example of evangelical interdenominational cooperation, the domestic "city mission" movement, which put hundreds of lay missionaries into London slums in the nineteenth and early twentieth centuries.

The leadership of the Church of England came to recognize in the nineteenth century that the rules governing religion had changed. Forced to compete in an institutional and ideological free market, the church responded by covering the nation with a network of voluntary religious institutions. Anglican and Nonconformist evangelicals, working together though usually on Anglican terms, created supplementary organizations to fill the gaps left by parish churches and schools and succeeded in recruiting far more slum workers than did their Anglo-Catholic competitors.

The city missions hired working-class and lower-middle-class men and women and sent them directly to working people who could not be reached by the traditional parish church and its university-educated clergyman. These domestic missionaries went door to door, visiting the poor in their homes, reading scriptures to them, distributing tracts, helping them find food, clothing, and medical care, and harassing them for their vices.

Lewis shows that the city missionaries, along with other ecclesiastical agents, succeeded in promulgating a vague, diffuse form of Christianity among the poor and contributed to a widespread and obtrusive religious presence in London's supposedly unchurched slums. But they failed altogether to recruit people into a positive commitment to the Church of England or to any other church.

Why did they fail on this last point when they succeeded at so much else? Here Lewis becomes uncertain and demonstrates once again the damaging influence of the sociological theory of secularization, which defines large classes of people in any modern industrial society as naturally resistant to religion. Lewis's speculation that there was a lack of demand for religion leads him away from his own extensive evidence pointing to serious deficiencies on the supply side.

Evangelical leaders hired working people to evangelize working people but shackled their agents with crippling restrictions designed to prevent the exercise of theological innovation, the infringement of clerical prerogatives, or interference with those market forces necessary to maintain virtue among the poor. Prominent among a city missionary's mandatory tasks was "reproving open vice." Harassing people for the way they live is a recruiting technique of limited value, especially when it comes from an outsider. Evangelical leaders made certain that their agents, however welcome to some, ultimately remained aliens to most slum dwellers.

This was partly a matter of social class but also a consequence of the characteristic nineteenth-century Anglican theory of church growth. Anglicans of all parties believed that church extension in a pluralistic society should be primarily institutional. Instead of recruiting individuals into the church and then providing institutions for the committed, they built institutions to provide a general religious and moral influence among those who were not committed and hoped that large-scale individual recruitment would follow.

Why they proceeded in this inefficient way, both at home and abroad, is a question that requires further investigation. A different kind of religion, a religion *of* the people rather than *for* the people, might have thrived in the slums of Victorian London, just as such religion thrives today in the slums of New York, São Paulo, and Lagos.

JEFFREY COX
University of Iowa

PETER R. H. SLEE. *Learning and a Liberal Education: The Study of Modern History in the Universities of Oxford,*

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Cambridge, and Manchester, 1800–1914. Wolfeboro, N.H.: Manchester University Press. 1986. Pp. x, 181. £25.00.

The development of history as a discipline is a complex story that involves the recovery and integration of intellectual, social, and institutional dimensions. Peter R. H. Slee's essay deals exclusively with the institutional development of history teaching largely at Oxford and Cambridge. Scholars have long agreed that, as Slee argues, the discipline of history was part of a liberal education. But Slee defines liberal education too narrowly as mental training. In two chronological narratives, from 1800 to 1867 and from 1867 to 1895, he covers the drafting of the curriculum and the establishment of Honours degrees in history at both Oxford and Cambridge. The concluding section, covering the period 1890 to 1914, concentrates on the successful defense of mental training in Oxford and Cambridge against those who argued that history students must be taught to do research. History became a technical subject, as Slee says, only in Manchester.

At Cambridge, Slee traces the teaching of history from its origins in the Moral Science Tripos in 1848 to its establishment as an independent tripos in 1873, based largely on J. R. Seeley's understanding of history as practical politics. Critics of the tripos, led by George Prothero, the first history tutor at King's College, achieved a compromise in 1885, which Slee claims endured until 1914. History became a recognized subject with its own procedures, while the older political and theoretical papers, designed to train politicians, were also retained. At Oxford modern history, law, and political science were put together initially as an optional School of Law and Modern History, taught and examined on the basis of a limited number of texts, which yielded facts whose objectivity was thought to forestall political or religious bias. Then, between 1867 and 1895, Slee contends that the Oxford History School changed from an exclusive reliance on texts to the study of periods and subjects, taught by intercollegiate specialist history teachers. But, until the 1930s and beyond, the heart of the curriculum was still set texts and "authorities," as the selected compilations of original sources were called, taught by college teachers whose special field was determined more by what they taught than by systematic study or independent research. In the combined lecture scheme of the 1860s, which led to the creation of a college tutor's Modern History Association, Slee sees the beginning of a historical profession in the English universities. But was that not part of the development of a larger teaching profession throughout the universities, especially after 1871 when dons could marry and have permanent careers as university teachers?

The book closes at the beginning of the twentieth century, when a "new school" of historians promoted the interdependence of education and research in careers based on scholarly publication. But J. B. Bury as Regius Professor at Cambridge and Charles Firth as his Oxford counterpart failed to alter teaching at the ancient universities, largely because of prevailing satisfaction with the existing systems, whereas T. F. Tout, "the right man in the right place at the right time" (p. 153), as professor at Manchester, succeeded in instituting technical training for undergraduates in his new university.

Slee's comparisons are not always successful in that he does not always compare similar things. Although he discusses the debates within Cambridge about their tripos, he does not deal with the sustained criticism of both the method and the content of history teaching by Oxford-trained history teachers such as A. J. Ashley, Richard Lodge, Ramsay Muir, and F. M. Powicke. And he does not appear to appreciate that a sufficient understanding of any institution is always problematic because of the interior depths concealed behind the public facade. By describing the establishment of a history curriculum at his three universities, Slee has tried to test generalizations about the late nineteenth-century professionalization of history. Slee correctly claims that university reforms replied to criticism of anachronism while preserving the traditional ethos of a classical and mathematical liberal education. What he does not consider is that the concept of a liberal education also included prescriptive values about conduct and character that evolved further during the nineteenth century in response to increasing national opportunities for university graduates to fill expanding public offices and influential positions. Although isolated and remarkably autonomous, the universities were part of a larger world that decisively affected not only the development of history as a university discipline but also the interpretation of that world by university historians.

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RICHARD SYMONDS. *Oxford and Empire: The Last Lost Cause?* New York: St. Martin's. 1986. Pp. xviii, 366. $29.95.

Of the two universities responsible for providing much of the leadership in Britain in the nineteenth and twentieth centuries, Oxford has always been more closely identified with empire than has Cambridge. Why this should have been so is not made entirely clear in this book, but there can be no doubt

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about Oxford's pervasive and extensive influence. Whether it be in Downing Street, New Delhi, or the remotest mission station, Oxford graduates predominated. Cecil Rhodes, Alfred Milner, George Curzon, and Lionel Curtis were all Oxford men, as were more than half the Indian Civil Service and the elite Sudan Political Service. Prophets, philosopher-kings, colonial administrators, district officers, missionaries, and teachers more often than not left Britain instilled with the idealism of T. H. Green and Benjamin Jowett and saturated in the history and literature of Greece and Rome. Rhodes's bequest only deepened, broadened, and complicated the ties. But Oxford spawned critics as well as acolytes, and Goldwin Smith, Sydney Olivier, Leonard Barnes, and W. M. Macmillan had honed their contrary skills in the Oxford tutorial.

Richard Symonds has undertaken to unravel this complex interconnection. After a brief introduction, Symonds divides his study into three sections. In the first, "The Perception of Empire," he glances quickly at the senior common room's perception of empire over the years as reflected in the *Oxford Magazine* before moving on to examine the ideas and methods of Jowett, Green, Curzon, Milner, John Ruskin, Arnold Toynbee, and Thomas Arnold. The historians of empire, Edward Freeman, James Froude, Hugh Egerton, Reginald Coupland, and Margery Perham receive a nod, and the Round Table movement is given a separate chapter. The section ends with a survey of the critics of empire. In the second part, "A Great Imperial University," Symonds examines Oxford's educational role. The fortunes and influence of Indian Studies and the Indian Institute are set out, and Oxford's efforts to promote the study of imperial medicine and forestry are given a cursory glance. Oxford's impact on the study of imperial geography and on anthropology, scholar-administrators, economics, and law is treated at greater length, and Rhodes and his scholarship scheme are given a chapter. The third section, "Oxford Overseas," initially examines the role of Oxford men in the various services (India, Colonial, Egyptian, and Sudan) and then in separate chapters considers Oxford's influence on missionary movements and education in the overseas empire. Symonds ends with a glance at the experience of imperial and commonwealth students in Oxford and at Oxford's involvement in the training of the colonial service in the last days of empire after World War II.

This is a richly informed and well-written book that all historians of the empire and commonwealth will appreciate. Its strength is the breadth of its coverage. Its weakness is its lack of analysis. Because of the extent of his canvas, Symonds can give only a few pages to each issue, topic, or individual, and the result is purely descriptive with but tantalizing glimpses of rich and complicated avenues of research begging for exploration. The conclusions are frustratingly indecisive, and Symonds admits that it may be too early to assess the extent and degree of Oxford's influence.

Nevertheless, there is much of value here for the student and the historian of the empire and commonwealth, and the book will probably spawn numerous offshoots as others explore more fully what Symonds could only touch on.

JOHN KENDLE
University of Manitoba

LANCE E. DAVIS and ROBERT A. HUTTENBACK. *Mammon and the Pursuit of Empire: The Political Economy of British Imperialism, 1860–1912*. Assisted by SUSAN GRAY DAVIS. (Interdisciplinary Perspectives on Modern History.) New York: Cambridge University Press. 1986. Pp. x, 394. $47.50.

Ever since J. A. Hobson advanced the theory that financial power was the mainspring driving imperialism (and that was in 1902), economic determinism has held pride of place among explanations for the expansion of the British empire in the nineteenth century. This book sets out to discover the extent to which Britain's prosperity in the period 1860–1912 derived from its economic and political relations with the empire. To test the hypothesis, Lance E. Davis and Robert A. Huttenback in separate chapters examine the flows of long-term finance through the London market; analyze the private rates of return on home, foreign, and empire finance; investigate patterns of government expenditure to see if they were set so as to favor British business; provide an estimate of the British subsidy to the empire in terms of defense expenditures and of other items such as interest, loans, and grants-in-aid; identify the investors in firms primarily doing business in the empire; distinguish the groups in Britain who actually paid the taxes that supported the imperial subsidy; and explicate the relations between Parliament and groups in Britain with an interest in empire, such as chambers of commerce. This is a formidable task. It is made even more daunting by the extensive use of comparative data from a wide range of foreign countries, both developed and undeveloped.

The organization of the book is extremely tight, with each chapter containing an introduction in which the problem to be examined is set out, followed by detailed analysis of the statistical evidence, and finishing with a conclusion in which the argument of the chapter is summed up. The authors conclude that with few exceptions the dependent empire (as distinct from the self-governing colonies) did not draw large quantities of British finance—a

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major challenge to the Hobson-Lenin thesis. They also argue, after examining the records of several hundred firms doing business in the empire, that at least after 1880 (when the heyday of imperialism supposedly began) the empire may have been good business for some companies but for most more profits could be made in the domestic market. On the question whether the British warped the political process in their dependent colonies in order to bolster the profits of British businessmen, the evidence is less conclusive but on the whole (even in India) does not support the charge. As for the subsidies, it is clear that imperial defense cost British taxpayers a good deal, since the self-governing colonies sedulously refused to contribute any significant share, and in other respects British taxpayers subsidized empire through such means as grants-in-aid, subsidies for steamship and telegraph communication, and guarantees of interest on bonds issued by governments in the empire. By contrast, at least in the self-governing colonies with their reliance on customs duties for a considerable proportion of their revenues, British business was penalized by having to pay the duties in the same way as foreign businesspeople. India, of course, was an exception in this regard. The authors end with a concluding chapter in which they sum up the arguments of the book: the British as a whole did not benefit economically from the empire, although individual investors did. Probably at no time, and certainly not after the 1870s, were profits from the empire sufficient to underwrite British prosperity, but for shareholders in a limited number of sectors (agriculture, extractive industries, and public utilities) the empire was important. These individuals were mostly the elites of British society (peers, landowners, financiers, military and civil officers, the clergy) and businesspeople in the City.

This is an important book and one that is certain to generate a good deal of discussion (much as Ronald Robinson and David Gallagher's *Africa and the Victorians* did twenty-five years ago). This is because of the challenge it presents to "received" theories of empire, particularly those inspired by the Hobson-Lenin thesis (those who support the "drain theory" as the basis of Britain's relations with India will not easily abandon their positions) and because the argument rests on a mass of quantitative data that will be subjected to critical scrutiny and undoubtedly different interpretations. Incidentally, since the book presents so many tables (just as one example, the discussion about voting patterns on imperial issues in the House of Commons is backed up by twenty-one consecutive pages of tables), it is a pity there is no list of tables at the beginning. Also, a Canadian reviewer cannot fail to point out the misspelling of Louis Riel's name (p. 151) and the complete confusion over the British meaning of billion (on page 71 the authors claim that sales of new stock issues in London in 1914 amounted to 192 billion pounds, and on page 255 we are told that in 1909 businesspeople in the City pledged 134 million pounds to the Liberal party's election fund) and to note there are other minor errors here and there. But these are small points. There is a great deal of interesting material in this book, from the high ground of the main argument to more mundane matters such as the war between Bovril and Oxo for domination of the bouillon cube market in England between 1877 and 1911. It will stimulate much discussion and send many historians to their battle stations.

PETER HARNETTY
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ERIC TAPLIN. *The Dockers' Union: A Study of the National Union of Dock Labourers, 1889–1922.* New York: St. Martin's or Leicester University Press, Leicester. 1986. Pp. xviii, 198. $25.00.

The dockers' union that covered the area from Glasgow to Merseyside has been overshadowed by more dramatic events and personalities in London. In this well-constructed narrative Eric Taplin has filled a major gap in British labor history. Despite the lack of official records, which were sold to a scrap paper merchant in 1956, he has drawn effectively on other sources to put together a detailed account of the development of the union from the beginning to its amalgamation into the newly formed Transport and General Workers' Union.

A central figure in the story is James Sexton, who headed the union from 1894 to 1922. Starting out as a militant spokesman for the grievances of the dockers, he soon developed into an archetypical "pragmatic, cautious trade union leader, constitutionalist and subtle tactician," not very popular with the rank and file. Taplin shows that throughout his career Sexton was determined to protect his membership against the sort of thing that he himself had experienced. At the age of twenty-six, while working on the Liverpool docks, he suffered an injury that disfigured him for life; his cheekbone was smashed, one eye dislodged, and his skull fractured; before being taken to the hospital, where he spent two months, he was left lying in a dock shed until the unloading was completed; he received no compensation from his employer, although defective machinery was at fault; when returned to work, one-fifth of his first week's earnings was deducted to pay for the cost of the cab that took him to the hospital. From this background emerged what may be called Sexton's "trade union consciousness," which brought him into conflict with James Larkin in 1911. There is a good chapter on their confronta-

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tion, which "epitomized the clash between revolutionary and reformist trade unionism" (p. 67).

Taplin notes that the history of individual unions has become less fashionable. In fact that makes this sort of study all the more useful. It is, in an old-fashioned phrase, an objective contribution to knowledge, solidly based on a careful assessment of the evidence. What has become problematic, however, as a result of work with a more theoretical and analytical orientation, is the nature of the structure of knowledge into which research findings have to be fitted.

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DAVID C. SMITH. *H. G. Wells: Desperately Mortal, A Biography*. New Haven: Yale University Press. 1986. Pp. xviii, 634. $29.95.

H. G. Wells continues to fascinate. This is demonstrated by the dozens of studies, general, specialized, pro or con that have been devoted to Wells's controversial life and work since his death in 1946. David C. Smith's admiration for his subject places him solidly in the pro school. He informs us that, looking back on his own life, it is difficult to remember when Wells was not a part of it, from the year in which he was given *The Time Machine* to read as a child, through his high school discovery of Wells's historical writings, which brought to life for him an otherwise boring subject, and on to an immersion in Wells's fictional writings as a postgraduate student, which set the stage for the eventual composition of this biography. The reader is left in no doubt that during this long period of authorly gestation Wells became for Smith a kind of prophetic cultural hero. Thus, we learn that, among other prescient or even prophetic things, Wells was a major if not the only influence behind the didactic novel, the tank, the U.N. Charter of Rights, the Beveridge Report, modern feminism, even perhaps the campaign to save the whales.

If Wells is, by Smith's or any standard, an undeniably compelling biographical subject—his fabled and highly publicized sexual promiscuity alone would guarantee this—he is also a problematic one. A human volcano of a figure driven by a massive appetite for life, he was to rise from lower-middle-class obscurity to become one of the most celebrated intellectual figures of the twentieth century—one of his books alone, *The Outline of History*, we are told, was the greatest seller of its time except for the Bible and the Koran. His habits of prolific authorship, his multifarious sexual liaisons, his extensive correspondence, and his wide embracement of public causes in the course of a long life in which he remained remarkably active until close to the end add up to present a daunting challenge to Wells's would-be chronicler. Undeterred by this, Smith set out, in adding his own lengthy contribution to an already large literature, "to provide a life in context, and the whole life" (p. xii). He indicates that the fulfillment of this aim involved him in some ten years of work in which he reviewed more than three thousand items of published material by Wells and perhaps as many as thirty-five thousand letters and documents in archives in the United States and in England.

Smith's book is divided into four roughly chronological parts. The first of these covers Wells's modest beginnings in Bromley, Kent; the formative intellectual experience of his discovery of T. H. Huxley and evolutionary science at what was to become Imperial College, South Kensington; his marriages, first to his cousin Isabel and then, lasting until her death in the 1920s, to the long-suffering, or at least endlessly indulgent, A. C. Robbins ("Jane"); and on to teaching and journalism and the first burst of futuristic fiction stretching in the 1890s from *The Time Machine* through *The Island of Dr. Moreau* to *The War of the Worlds*. Part 2 covers that extraordinarily creative period, down to World War I, during which Wells published such major didactic novels as *Kipps*, *Tono-Bungay*, *Ann Veronica*, *The History of Mr. Polly*, and *The New Machiavelli*, while flirting with the Fabians and advocating a brand of evolutionary, scientifically visionary, and non-Marxist socialism in which female equality loomed large.

In part 3 of Smith's book the focus shifts from Wells's involvement in specifically British affairs to his self-anointed role as the author of "textbooks for the world," in which he was eventually to urge an "open conspiracy" of the scientifically enlightened few against the darkening barbarism of a tumultuous century. Part 4, entitled "Prophet," as well as discussing Wells's propaganda on behalf of a global declaration of the rights of man and his continuing literary and journalistic activities during his declining years in London's Hanover Terrace in the midst of World War II, passes more fully in favorable review his feminism as reflected in his fiction and in the available testimony of the many women in his life, including such important affairs as those with, among others, Amber Reeves, Elizabeth von Arnim, Dorothy Richardson, Rebecca West, Odette Keun, and Moura Budberg.

In a short notice of so large a book, much must inevitably be excluded. Smith has given us a work of partisan scholarship painstakingly and carefully constructed. There are occasional slips—such as the suggestions that William Beveridge was a Labour party politician, that Alfred Noyes was a "Nazi," and that birth in Bromley rendered Wells a cockney—but they are few in number in a book the strength of

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which is its comprehensiveness. This biography cannot be said to have superseded earlier studies of Wells's life, but it now assumes its place as an indispensable companion to them. The life of one who, as Smith himself says, was an intense bundle of force and desire resists final encapsulation within the covers of a single book. And biographical evidence that is garnered out of an unstinting admiration for Wells's exceptional achievement not infrequently has the unintended consequence of revealing not only the soaring vision but all the more vividly the anomalies, inconsistencies, and, on occasion, double standards of a genius almost childlike in its indulgence of a passionately desiring self. H. G. Wells indeed continues to fascinate. And Leo Amery was entirely correct, and more insightful perhaps than he realized, in once describing him as a "free and irresponsible artist."

GEORGE FEAVER
University of British Columbia

A. R. PETERS. *Anthony Eden at the Foreign Office, 1931–1938*. New York: St. Martin's or Gower, Aldershot, U.K. 1986. Pp. viii, 402. $39.95.

Fifty years ago Anthony Eden resigned as British foreign secretary and briefly entered the wilderness of the back benches. The path from his first Foreign Office appointment in 1931 to the break with Neville Chamberlain is described in great detail in this study, which lacks an explicit thesis but conveys a strong impression of a man more influenced by his experiences in various Foreign Office missions than by either prescient understanding of the aims of Mussolini and Hitler or moral revulsion against dictatorship. In that, Eden was clearly in the mainstream of British diplomatists of his day, and his resignation in early 1938, like his earlier willingness to try appeasement, was undertaken reluctantly rather than defiantly. The title Eden gave to his memoirs of the 1930s, *Facing the Dictators*, hardly fits the events and positions recalled here, which show Eden moving only gradually and tangentially toward any real confrontation with the fascist powers. Undoubtedly, his resignation was an act of principle, but A. R. Peters also presents a picture of a man so popular with the Tory rank and file that relegation to any permanent wilderness was most improbable.

Unlike interpreters of Eden's development who stress his personal strengths or weaknesses, such as Randolph Churchill and Robert Rhodes James, Peters focuses on his rise within the Foreign Office, showing how the briefs he was given there (especially responsibility for relations with the League of Nations) shaped his views both of the West European powers and of the cabinet members around him. It is useful to be reminded that well into 1938 the foreign policy orientation of the governments of Stanley Baldwin and Chamberlain concentrated more on France and Italy than on Germany and that to Eden's mind as well as that of the cabinet's inner circle Mussolini held the key to European peace. The internal memorandums and correspondence Peters cites reveal that in the mid-1930s Eden staked his position on two assumptions: that he could convince his colleagues of the folly of trying to lure Mussolini away from Hitler by caving in to Italian demands and that Britain could persuade the United States to intervene to right the balance of power in Europe. On the first score Eden proved unable to overcome the stubbornly confident Chamberlain, the indecisive Baldwin, or the rather pro-German Cranborne (later Halifax). On the second, he showed himself to be naive and badly informed about American politics, as indeed he still was two decades later in launching the Suez War.

The author chronicles these events carefully, in flat and functional prose of the kind Eden himself favored. (Oddly, the most vivid and direct writing in the book comes in Chamberlain's letters to his sisters.) The emphasis is on outcomes rather than motivations, no doubt because it is extraordinarily difficult to penetrate the inner life of the Foreign Office in this (or any other) period. The worlds of Cliveden, of *The Times*'s editorial staff, or of a Britain in the depths of the depression hardly figure in this account, certainly not for Eden caught up in the rarified world of cabinet and Foreign Office. In that early period of his career, when the pulls of ambition and of conscience were struggling under the cool, handsome veneer, Eden's life-long and ultimately short-sighted preoccupation with foreign affairs was already under way. This book is a useful chronicle of that troubled apprenticeship.

JAMES B. CHRISTOPH
Indiana University

OWEN CHADWICK. *Britain and the Vatican during the Second World War*. (Ford Lectures.) New York: Cambridge University Press. 1986. Pp. ix, 332.

This book grew out of the Ford lectures that Owen Chadwick delivered at Oxford University in 1981, and it provides a valued corollary to the work already published by the doyen of Vatican diplomacy, Robert Graham. Lucidly written and exhaustively researched, Chadwick's pioneer study offers evaluative judgments of not only the recently opened British government records but also hitherto unconsulted Italian archives. The principal characters of this story are the British minister to the Holy See, d'Arcy Godolphin Osborne, and Pope Pius XII.

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Osborne, who was accredited to the Vatican from February 1936 through June 1947, kept a diary, which Chadwick skillfully employs to expand on the descriptive accounts contained in the British diplomatic correspondence. This private chronicle, together with the official telegrams, letters, and dispatches, offers an instructive account of the inner workings of the Vatican and its relations with Benito Mussolini's government during World War II. The British minister was the only diplomat of a major Allied power to remain in the sanctuary of Vatican City for virtually the entire war, and his perceptive assessments afford an invaluable record for anyone interested in the study of Fascist Italy or the Holy See.

Life was often difficult for Osborne owing to the strain of sustained confinement and general isolation, not to mention the deliberate discomforts caused him by the pro-Fascist Vatican City chief administrator, Nicola Cardinal Canali. The minister's primary responsibility was to explain or defend British policies at the Holy See while simultaneously seeking to win Vatican support for the special interests of the London government. Neither of these objectives was much helped by the Italophobic attitude of Foreign Secretary Anthony Eden or by the tendency of Osborne's superiors to recognize the legitimacy of the Lateran Treaty only when it suited their own purposes to do so. Throughout his many trials and tribulations, however, the British minister acquitted himself with such tact and skill that he won the esteem and confidence of nearly every member of the Vatican hierarchy and was later thanked by name in a public tribute in the House of Commons.

Pius XII emerges from this book as a saintly man for whom Osborne, a Protestant, had great affection. Chadwick does not attempt to address the question of precisely what role the pope might reasonably have been expected to play during World War II, but he does present Osborne's account of two rather exceptional papal initiatives. The first was in early 1940 when Pius XII passed information on to London from conspirators in the German army who were prepared to overthrow Hitler in return for certain safeguards that were to be guaranteed by the Vatican, and the other was late in 1943 when the pope cooperated with parties attempting to secure an Italian armistice in the war. Yet, there were instances when Osborne would be deeply dismayed by the public rectitude of the papacy in the face of outrageous Nazi atrocities.

Readers will be fascinated by this British minister to the Holy See who could lecture government leaders and scold Vatican prelates, but who sometimes was intimidated by his manservant. Chadwick's evident command of the ecclesiastic and historical dimensions of the often complex but always important Anglo-Vatican relationship during World War II makes this a seminal work for diplomatic and church historians alike.

THOMAS E. HACHEY
Marquette University

JAMES CABLE. *The Geneva Conference of 1954 on Indochina*. New York: St. Martin's. 1986. Pp. xii, 179. $32.50.

This slender volume examines "British attitudes towards the conflict in Indochina . . . British motives for promoting the Geneva Conference, and . . . the conduct of British diplomacy, not only at Geneva itself, but in the various meetings, discussions and confrontations that preceded the Conference and continued, in different places, while the Conference was in progress" (p. 2). Does this limited perspective on a meeting that failed in its ultimate objectives justify a book? The conference did help end the long and bitter war in Indochina, enabling French troops to return home and their enemy, the Vietminh, to pull out of Cambodia and southern Vietnam behind negotiated lines of demarcation. But, if these military agreements worked fairly well, the rest of the Geneva accord broke down quickly.

James Cable's main thesis, that British efforts were critical in allowing the conference to be held and to achieve some success, may be overstated. However, his book is a valuable contribution to the already considerable body of literature on Vietnam. It uses newly released British documents to good effect and offers the unique perspective that the author, a member of the British delegation at the conference, can provide. His anecdotes and deft portraits of Anthony Eden, Georges Bidault, and other leaders enrich the account.

Indochina was not at the top of the diplomatic agenda in London in 1954 as it was in Paris. It took on importance for Prime Minister Winston Churchill, failing in health and concerned that the Soviets had just gotten the hydrogen bomb, as a step toward a summit meeting and lasting peace. But could the Americans, and especially Secretary of State John Foster Dulles, who "considered neutrality immoral" (p. 85), be persuaded to accept a compromise solution in Indochina? In the long run, they could not, but in 1954, although they blustered and threatened to use atomic weapons in Indochina to get their way, they did give tentative support to the Geneva deliberations. This enabled British Foreign Secretary Eden to craft a modest foundation for peace in Indochina, which compared quite favorably with the agreement that Henry Kissinger was able to reach with the North Vietnamese two decades and too many lives later.

How important was the Geneva Conference of 1954 then? Less important than Eden thought.

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Cable notes ruefully that Eden may have gotten an exaggerated sense of his own and Britain's ability to determine world events, which led him to disaster in 1956 at Suez. Still, for Britain, and of course for France, the conference provided a way out of military involvement in Indochina for which they could be grateful. The North Vietnamese, who felt they had been cheated at Geneva, later blamed the Chinese—unjustly, Cable demonstrates—for their plight. When the occasion presented itself, they were ready to return to the battlefield to set things right. The United States, with a "passion Europeans could not prevent [for] the immolation of Indochina," provided that occasion (p. 144).

DONALD S. BIRN
State University of New York, Albany

HELEN KELSALL and KEITH KELSALL. *Scottish Lifestyle 300 Years Ago: New Light on Edinburgh and Border Families*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1986. Pp. vii, 224. £10.00.

This may well be the first time that the words "style" and "Scottish" have appeared in the same title, and it was with some anticipation that I watched to see how the concept of "life style" could be useful to the social historian. My disappointment here did not mean that Helen Kelsall and Keith Kelsall's book was an entire failure. There is a wealth of material in their sources—a diary, autobiography, account book, and letters relating to a middle-to-upper gentry family in Berwickshire around 1700. Using local tax records, the authors saw the possibility of giving a rounded view of life at that level of border society.

Maybe there was too much material, in fact. After summarizing it, the authors did not feel the need to squeeze to make it yield more. What the book lacks is any sense of what we want to know, as opposed to what the documents meant to tell. Inevitably, the work invites comparison with Alan Macfarlane's study of a clergyman's diary of the same period. Macfarlane often had to admit that his anthropological concepts were not readily applicable to English culture. But at least they helped him formulate an agenda and make maximum use of his sources. There is no such agenda in the present book.

But there are certain advantages in the Kelsalls' approach. Analysis can make for heavy reading, whereas this more superficial description is reasonably entertaining. We imagine that we are viewing things as contemporaries were conscious of seeing them and that we know what they thought was important. But we are not forced to wonder why they recorded what they did. We do not learn things of which the subjects were themselves unaware or find the patterns that shaped their minds.

The one control exercised over the study is its restriction to a single family and its social ties. The authors do not browse over large patches of time or locale to find the picturesque or the surprising. And yet we still learn a little about a lot of things as they existed in a single household: funerals and marriages and birthing customs, reading and eating preferences, silver, prices, crime, medicine, recreations, socializing, servants, taxes, family quarrels, coaches, postal service and news, inheritance, "superstitions," health, wigs, and dress. We do not penetrate to the essence of a "style" or even see things in contrast to England, for the authors show no acquaintance with any studies of English society.

Finally, there is no argument that the family was either exceptional or typical. But, in fact, there was far too much drama in the lives of the Humes (or Homes) of Polwarth for them to be representative of "Edinburgh and Border Families." From penurious exile in Utrecht, where they had hidden from James II, Patrick and Grisell came back to wealth, an earldom (of Marchmont), and high office under William III. Understandably, the tendency toward narrative rather than analysis eventually overcomes the authors, for it makes a good story. In the end it is an account of life purpose, rather than life style—which somehow seems more fitting for Scotland.

C. JOHN SOMMERVILLE
University of Florida

T. C. SMOUT. *A Century of the Scottish People, 1830–1950*. New Haven: Yale University Press. 1986. Pp. xiv, 318. $25.00.

The long-awaited publication of this book is a significant event in both the historiography of modern Scotland and the career of T. C. Smout. In 1969 he published *A History of the Scottish People, 1560–1830*, the work on which his high reputation was built. Smout came to be regarded as the man who had more or less invented modern Scottish social history, and for his pains he became the recipient of both extravagant praise and a significanct measure of ill will. The praise was justified by the courage with which a young man had tackled a huge sweep of complex early modern history and, admittedly greatly helped by the extensive edited source collections produced by Scottish historical clubs, had drawn the topic together in an elegant, lucid prose inspired by what were then the best contemporary practices in social history. Ill will was inevitably generated by the too ready assumption of some of his Oxbridge reviewers that this was the first "modern" monograph in the field of Scottish history, an assumption that the work of the present historiog-

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rapher royal, Gordon Donaldson, alone would have disproved. There was also a feeling among members of Scottish history departments that what they were looking at was a clever textbook that achieved impact by slightly oversimplifying the easier half of its field. "He will not find the second volume so easy" was the remark of one, the thought of many.

Nor did he. With the development of heavy industry, the spectacular acceleration of urbanization, traumatic consequential change in the countryside, and galloping social, cultural, and ethnic pluralism, nineteenth- and twentieth-century Scotland becomes an almost impossible country on which to craft those elegant generalizations about, for example, the culture of the Enlightenment, that had added panache and readability to the first volume. Then one has to realize that Smout was caught by the flood of aggressive quantification that deluged economic history departments in the 1970s. Even in social history the rules of the game had changed, and there were some very unpleasant people around ready to denounce with bell, book, and candle anyone who failed to accept their definition of the right approach. With a precious reputation to lose, it is hardly surprising that Smout took his time.

Always a fine exponent of the art of the learned article (of which he must have written over forty by now), he embarked on a whole series of detailed studies preparatory to this volume. It is good to see the substance of the best of them, such as his beautiful piece on Paisley in the depression of 1841–43, embedded in this book. He also produced work on demography in Michael Flinn's big Edinburgh-based corporate assault on Scottish population history and a very technical study based on census material relating to the Scottish working class in 1843. Of course, he is far too clever to make the mistake of writing a parallel volume to that of 1969. That remit was indeed impossible. What he has done is write a splendid series of essays on the history of the Scottish working class to form a sequel to, rather than a second half of, his previous work. The technical scholarship is if anything better than that in the 1969 volume, and the approach is sparklingly fresh. Apart from the basics of work and wages, there are marvelous chapters on religion, alcohol, sex, and radical politics. By biding his time, Smout has been able to draw on recently completed major surveys of such massive topics as the Highland Clearances and Scottish education in the Victorian era. Yet he is always his own man, pointing out, for example, that the southern Uplands were emptied of population on as drastic a scale as the Highlands.

A big grantsman, Smout has projects currently running on Scottish prices and wages and on Scottish early modern diet. Both will no doubt produce important publications. His heart does in fact seem to lie in the earlier period, and, now that he has completed the second stage of his social history, one would like to think that he might be persuaded to go back to produce the revised version of his first volume that its own stimulating effect on the subject has rendered necessary. Anyone who has the wit and courage to point out that the rightist Labour party local councils that ruled so much of Scotland until recently were about subsidized housing and sectarian education (not to mention contracts) and had absolutely nothing to do with socialism, let alone democracy, can look modern historians in the eye for the rest of his life. A commitment has indeed been honored.

BRUCE P. LENMAN
University of St. Andrews

JAMES LOUGHLIN. *Gladstone, Home Rule, and the Ulster Question: 1882–93*. Atlantic Highlands, N.J.: Humanities. 1987. Pp. 369. $39.95.

According to James Loughlin, James Bryce's post-1885 election report that Ireland was on the brink of social disintegration was the final persuasion in W. E. Gladstone's conversion to Irish Home Rule. Once committed, he summoned history to its defense. The prime minister emphasized the long preunion Irish parliamentary tradition. He focused on W. E. H. Lecky's study of Grattan's Parliament to highlight the Protestant character of the eighteenth-century Irish nation. Gladstone argued that the union had magnified class and religious divisions in Ireland. He said that the restoration of an Irish legislature would be a conservative solution to the Irish Question. Irish Catholics would take the direction of a mostly Protestant aristocracy and gentry. Gladstone and the Irish Parliamentary party favorably compared eighteenth-century Protestant patriotism with the Irish unionism of their time, branding the latter as narrow Orange factionalism. When nationalists came to realize the almost unanimous Ulster Protestant antipathy to Home Rule, they rejected it as only a portion of the province's geography and population. Gladstone, however, continued to discount Ulster unionism as a selfish contradiction of the Grattan tradition. Since they were more interested in preventing Home Rule than in separate status for themselves, Ulster Protestants did not put forward a specific partition proposal. Despite threats of violence, they planned to resist a Dublin government passively before employing physical force.

Loughlin goes beyond Ulster in charging Gladstone with misreading the Irish situation. Gladstone's praise of Grattan's Parliament underplayed the exclusive quality of the Protestant nation, blurring the distinctions between eighteenth-

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century Irish patriotism and essentially Catholic nineteenth-century nationalism. It emerged out of the agitation for Catholic emancipation and was fueled by the long struggle against Protestant landlordism. Consequently, Irish Protestants viewed the union as their shelter, and those in the industrial north considered its preservation necessary to economic survival. Loughlin insists that Gladstone overestimated Irish Catholic enthusiasm for Home Rule. Their support for the Irish party represented gratitude for its constituency efforts, especially in regard to the land question, rather than any passion for its vague notions of what Home Rule involved.

Loughlin's critique of the contents of Gladstone's Home Rule measures accents their financial limitations. Imperial contributions would have so handicapped the Irish economy that they would have intensified rather than diminished anti-British feelings in Ireland. Charles Stewart Parnell and other Home Rule M.P.'s objected to the financial clauses but had little choice but to accept Liberal offers. According to Loughlin there was a possible positive consequence of the financial injustices in the bill of 1893. If it had passed through the House of Lords and become law, Dublin would have had to overcome Ulster passive resistance with conciliation to achieve a stable economy.

Among the strengths of Loughlin's book are clear exposition, an interesting analysis of the factors that influenced Gladstone's embrace of Home Rule and the historical defense of his position, an examination of the complexities and self-deception of the prime minister's mind and its misinterpretation of Irish realities, an intelligent study and criticism of the first two Home Rule bills, and a probing if sympathetic presentation of the Ulster unionist point of view. The book's weaknesses include the contention that Home Rule had little favor among Irish nationalists. It is supported by a scanty supply of newspaper comments that complain of the contents of Gladstone's proposals rather than the Home Rule principle. Loughlin's conjecture that Protestant Ulster in time might have accepted the bill of 1893 is just too uncertain. Following the lead of other historians, Loughlin puts too much emphasis on the insensitivity of Gladstone and the Irish party to Ulster unionism. The alternatives the unionists proposed were denial of Home Rule or placing a large Catholic minority in a truncated Ulster under the domination of a paranoid majority. Political realities at the time and later events made the first choice impractical. After 1920 the second was tried with disastrous results.

LAWRENCE J. MCCAFFREY
Loyola University
Chicago, Illinois

TREVOR WEST. *Horace Plunkett: Co-operation and Politics, an Irish Biography*. Washington, D.C.: Catholic University of America Press. 1986. Pp. xviii, 288. $29.95.

In this well-crafted study, Trevor West re-creates the life and career of Horace Plunkett, the Anglo-Irishman who facilitated the modernization of Ireland's principal industry: agriculture. In doing so, West reveals the interaction of Plunkett's social, economic, and political philosophies, which set him apart from his fellow Irishmen and have made him a subject of enduring interest. Drawing on printed and archival sources in Ireland, Scotland, and Wyoming, plus Plunkett's diaries and papers at the Plunkett Foundation, Oxford, West presents a book that should be required reading for anyone interested in Ireland during the late nineteenth and early twentieth centuries. It reveals an important dimension of these years that had lasting impact on the course of Irish economic history, and it is a major addition to the growing body of related literature that has appeared since Margaret Digby's 1949 study of Plunkett.

As an Irish landowner, English born and educated, Plunkett dedicated his career to the improvement of Irish rural life. Widely traveled and an unorthodox unionist in politics, he launched the Irish Cooperative Movement because he believed that the longstanding dispute between landlords and tenants was nearing solution. Henceforth, in his view, the Irish should concentrate on efficient methods of agricultural production and distribution. As a member of Parliament, Plunkett chaired the Recess Committee, which met between sessions during 1895–96. This body laid the groundwork for Ireland's Department of Agriculture and Technical Instruction, a government agency that Plunkett administered from 1900 until 1907. From 1894 until his death in 1932, he headed the Irish Agricultural Organization Society, which fostered the spread of cooperative societies throughout Ireland.

Although dedicated to Ireland, Plunkett originally saw no reason to repudiate England. He viewed the Irish question not as a matter of politics, religion, or economics but as a problem of Irish character. He believed that self-government should follow, not precede, social and economic development. This theme underscored his early writings, and he looked to the landed gentry to help shape the new Ireland that was emerging, following the passage of the Wyndham Act in 1903. Their indifference eventually moved him to support Home Rule, and England's insensitivity to Ireland's needs led him, in time, to greater understanding of Sinn Fein and to acceptance of the Irish Free State. Plunkett believed that the Irish must work together across sectional, political, and sectarian lines to de-

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velop the self-reliance required for political and economic independence. Only in this way could they build a strong nation. This was one of the goals of his Cooperative Movement, but the inflexible posture of competing groups in Ireland prevented such an accomplishment, and the troubles that led to partition and civil war are well documented.

Plunkett's unique style and eccentricities often made him the subject of amusement and derision, but West argues that his opponents really saw him as a dangerous threat. Few in Ireland have experienced the repudiations he suffered. Fellow unionists staged the loss of his parliamentary seat in 1900, nationalists drove him from his position with the Department of Agriculture in 1907, and Edward Carson, the "no surrender" unionist, attacked him for advocating Dominion Home Rule. The final humiliation came when Republicans destroyed his home during the Irish Civil War in 1923. West suggests that Ireland had not matured enough within Plunkett's lifetime to accept the constructive self-criticism he offered.

As an aristocrat, an agnostic, a social and economic reformer, and a cosmopolitan, Plunkett was unpopular with many segments of Irish society: Gaelic nationalists, the clergy, Home Rulers, unionists, and the landed gentry. Through it all he held his ground, and through his books, his newspapers, and his firm dedication to his cause he brought important long-range changes to rural Ireland—changes that have left a permanent imprint on the land.

In retrospect West sees merit in Plunkett's early opinion that Ireland needed social and economic development before political independence. He sees little but stagnation in Ireland during the first half-century of self-rule and finds many of the character defects Plunkett complained of still present—a harsh judgment that undoubtedly will provoke response.

West's choice of quotations adds flavor to his narrative, but the reader wearies from their length and frequency.

VIRGINIA E. GLANDON
University of Missouri, Kansas City

CLARE O'HALLORAN. *Partition and the Limits of Irish Nationalism: An Ideology under Stress*. Atlantic Highlands, N.J.: Humanities. 1987. Pp. xviii, 243. $35.00.

The conflicting aims of sovereignty and unity—the fundamental contradiction in nationalist ideology—first confronted the founders of the modern Irish state in practical terms when they negotiated the Anglo-Irish Treaty in December 1921. Alternately cajoled and threatened by David Lloyd George, they unwittingly opted for sovereignty, thereby making partition a permanent fixture in Irish life. This study shows how the southern Irish public and political figures during the next two decades reconciled their continued ideological aspirations for unity with the unfolding realities of partition. It is an outgrowth of a master's thesis, but Clare O'Halloran shows none of the qualities of a novice, and this book should not be taken lightly. She brilliantly describes a phenomenon that scholars have long suspected but, until now, have failed to articulate adequately: that, however much rhetoric has been devoted to the concept of unity in posttreaty Ireland, virtually nothing has been done to achieve it. Furthermore, she shows that the attitudes and policies of the southern Irish have actually served to reinforce partition. Little wonder that nationalist ideology was under stress!

O'Halloran first explains how southern nationalists have traditionally viewed northern Ireland as "a place apart" and constructed stereotypes of the northern unionist as a hard-headed businessman and bigoted Orangeman "with a bowler hat crammed down on his ears," in the words of James Winder Good (p. 45). Such unendearing images hardly contributed to unifying gestures. But they did, as the author argues, aid in "fostering nationalist self-esteem and with coping with the trauma of partition" (p. 56). Even more striking is the southern view of northern nationalists as "foreigners" and "British helots," again employed as a rationalization. Especially after the boundary commission fiasco in 1925, republicans and moderates alike largely repudiated any responsibility for the welfare of their northern conationals. Stress was intensified not only by this casting of northern nationalists "to the wolves" but by the obvious dissimilarity in treatment of northern and southern minorities. Relying largely on political and journalistic sources, with a dash of literary flavor, O'Halloran's narrative has a patter not unlike Paul Fussell's *The Great War and Modern Memory* (1975) or Martin Wiener's *English Culture and the Decline of the Industrial Spirit* (1981).

The final section, based largely on official documents, is more deliberate in pace. It explicitly reveals how political practices in southern Ireland were increasingly at variance with the nationalist creed and antipartition rhetoric. The one blind spot in O'Halloran's analysis is that she fails to recognize that the precedent for this dichotomy was actually set in the treaty negotiations of 1921 in which Irish delegates were deluded into thinking they had obtained immediate sovereignty and ultimate unity. But such recognition only serves to reinforce her overall thesis and her specific assertion that the Irish government staked its life during the Irish Civil War and its aftermath on maintenance of the treaty. "Since the Provisional Government was forced to go

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out on a limb for the Treaty, all policy, had, on principle, to be construed in relation to it" (p. 103). Subsequent policy was formulated more on the basis of what was good for the Irish Free State alone. Economic and financial, not nationalistic, considerations seemed paramount. Even the emergence of Eamon de Valera's Fianna Fail party, despite its irredentist rhetoric, brought scant progress toward a northern solution. Gaelicization and the special place afforded the Catholic church in the constitution of 1937 further militated against it. Put simply, the nationalist "ideology was inadequate for the realities it faced" (p. 172). A useful epilogue shows how southern attitudes and platitudes have persisted into the era of the "troubles," Sunningdale, and the New Ireland Forum.

O'Halloran's account breaks new ground in Irish studies by picking up where John Bowman's *De Valera and the Ulster Question, 1917–1973* (1982) left off. It is bold, imaginative, and iconoclastic in the way it exposes hypocrisy in the modern Irish state. It will doubtlessly offend some readers, but it is a book that cannot easily be ignored.

JOHN D. FAIR
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JEAN-PIERRE BABELON. *Paris au XVI^e siècle*. (Nouvelle histoire de Paris.) Paris: Bibliothèque Historique de la Ville de Paris; distributed by Hachette, Paris. 1986. Pp. 626.

Like earlier volumes of the *Nouvelle histoire de Paris*, this book is lavishly illustrated, handsomely produced, and academically sound. In five hundred pages of text, Jean-Pierre Babelon surveys the nature and character of the sixteenth-century capital, its social and economic matrices, and the effects on it of the French religious wars. Fifty pages of reference materials provide additional information on the city's sixteen administrative *quartiers*, its parish churches and colleges, and its personnel. The forty-page bibliography contains over six hundred entries.

Predictably, the book is strongest in the areas of urbanism, architectural history, and the residential environment—areas in which Babelon is able to draw most directly on his own substantial contributions to Parisian history. These chapters convey a strong impression of the expansive force of the evolving city of the Renaissance. Babelon argues convincingly for the gradual implantation of a new urban aesthetic born of the Italian Renaissance and imposed by insistent royal pressure on an urban elite still comfortable with the enclosed spaces and Gothic aesthetic inherited from the Middle Ages. From the rebuilding of the pont Notre-Dame after its collapse in 1499 to the construction of the Hôtel d'Angoulême (now the Bibliothèque historique de la ville de Paris) in 1585, Babelon demonstrates the gradual evolution of a new urban style characterized by increasing preoccupations with order and symmetry and an eventual taste for Italianate architectural details. Significantly, he argues that, with the exception of the worst periods of crisis, the religious wars did not bring a halt to the city's impressive building activity. Also significant is his observation that the elaborate decorative schemes for the royal entry ceremonies played a key role in the evolution of taste.

The dynamism of the Renaissance city is apparent not only in its architectural evolution but also in its population and growth. In my opinion Babelon overestimates the size of the capital when he places its sixteenth-century high at three hundred fifty thousand, but he presents a good summary of both recent and traditional attempts to gauge the city's population, and, in fairness, he does stress the uncertainty of these figures. In the chapter on economic affairs, the gaps in current historical knowledge make themselves felt in a more indirect way. As might be expected, Babelon draws heavily on the important work on prices and salaries by such scholars as Micheline Baulant, but it is apparent from his more cursory treatment of corporative life and commerce that much monographic work remains to be done in these areas before we can have any thorough assessment of the urban economy.

The last section of the book, the account of the religious conflicts in Paris, also reveals the strengths and weaknesses of recent scholarship. It is strongest in its first and last chapters, where Babelon has profited from recent interest in popular piety to sketch a good picture of traditional, pre-Reformation Catholicism in Paris and from the new studies of the Holy League by Robert Descimon, Elie Barnavi, and others. The middle chapters on the Reformation and the Wars of Religion are less fresh; this period still awaits reappraisal. On the whole, however, the book is an excellent synthesis. It is a readable urban history and a valuable reference tool.

BARBARA B. DIEFENDORF
Boston University

DENISE TURREL. *Bourg-en-Bresse au XVI^e siècle: Les hommes et la ville*. Foreword by PIERRE GOUBERT. (Cahier des Annales de Démographie Historique, number 3.) Paris: Société de Démographie Historique. 1986. Pp. 290. 150 fr.

Bourg-en-Bresse, a small fortified city of four thousand inhabitants located some sixty kilometers northeast of Lyons, did not experience the dyna-

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mism so typical of sixteenth-century French towns. Bourg served as a judicial and administrative center and as a frontier fortress. Grains, unfinished textiles, and spun thread trickled southward to Lyons, but the town was too remote to participate fully in the expansion of the Lyons region. Indeed, despite the presence of a sizable military force, Bourg's population dwindled in the second half of the sixteenth century. If Bourg experienced little good fortune in the sixteenth century, it nevertheless bequeathed to historians a veritable treasure-trove of documentation suitable for a full-scale demographic study.

Bourg was part of the Duchy of Savoy until France annexed it in 1601. Fortunately for historians, the dukes of Savoy followed Italian administrative practices rather than French, and it is they who were responsible for the remarkable wealth of sources. Denise Turrel has made full use of these sources to reveal a demographic system that was quite different from the Old Regime model of the seventeenth and eighteenth centuries.

The nuclear family predominated, but the Old Regime pattern of postponed marriage and diminished marital fertility did not exist in sixteenth-century Bourg. Fully half the women were married by the age of twenty to men in their midtwenties. Premarital conceptions were very rare, but births within marriage occurred one after another at intervals of less than two years. For marriages that ran their full reproductive course, total births were in excess of eight children per couple, but only one-third of the marriages were reproductively complete. High adult mortality insured that two of three marriages ended long before the onset of infertility. Consequently, families averaged only four births, or half that of "complete" families. Remarriage was normal. Half the widows found new mates within a couple of years. Widowers remarried with greater ease and much greater haste; half the widowers remarried within three months.

Haste, rather than postponement, was the hallmark of this society. Early marriage, high fertility, and a low incidence of premarital conceptions were part of a sixteenth-century demographic system in which young adults were anxious to get on with life. For most people life would be all too short.

The great regulator was mortality. Mortality crises were frequent and every bit as severe as those of the seventeenth century. Epidemic disease was the principal cause of death during a demographic crisis, but devastating famines appeared too. Various ducal and municipal authorities made sure that adequate food supplies were available for Bourg's civilian and military population, but the rural poor who swarmed to the town in hopes of finding nourishment were systematically expelled. These miserable wretches died quite literally by the hundreds outside the walls of the city.

The demographic system that we see in sixteenth-century Bourg was in a transitional phase with clear, but slow, movement toward the more familiar Old Regime model of postponed marriage. Average age of marriage for women was just over 20 in the 1560s but 22 by 1619; age for grooms moved from 25 to 26. This fits nicely into the French pattern. As a rule, women married at age 21 in 1600, at 25 by the late seventeenth century, and at 27 on the eve of the revolution. The rising age of marriage was apparently a response to deteriorating economic conditions for the majority of the population. Population expansion in the sixteenth century drove real wages down. Economic growth was not sufficient to absorb the burgeoning population. Young adults found it more and more difficult to establish new households in an economically congested world. They had to wait a little longer before marriage.

Turrell's short and solid monograph is a valuable contribution to the history of the origins of the early modern demographic system. Turrell's study is all the more welcome because sources reaching back into the sixteenth century are extremely rare.

JAMES L. GOLDSMITH
University of Oklahoma

MICHEL ANTOINE. *Le dur métier de Roi: Etudes sur la civilisation politique de la France d'Ancien Régime.* Foreword by PIERRE CHAUNU. (Histoires.) Paris: Presses Universitaires de France. 1986. Pp. 343. 185 fr.

Michel Antoine's latest book is a collection of eleven essays, published over the last twenty years, on the royal institutions and administrative history of Old Regime France. The essays are a good introduction to the work of Antoine, who is best known for his studies of French royal councils and government in the eighteenth century. They are arranged chronologically and divided into two parts. The first part consists of six essays on royal administrative institutions, beginning with the chancellery in the sixteenth century, the central financial administration from the sixteenth to the eighteenth centuries, the concept of subdelegation, the redistribution of the *taille* from 1623 to 1625, the subdelegates of the intendants, and the intendants of Lorraine in the eighteenth century. The second part contains five essays based on administrative documents, beginning with the conflict between Louis XV and the *parlements* in 1767, the political trial of *parlementaire* Louis-René de Caradeuc de La Chalotais in 1766, the will of Louis XV, the bastards of Louis XV, and a concluding essay on the future of research on French institutions. The essay on subdelegates is the longest in the book and discusses the rapid growth

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in numbers and administrative functions of these officials, who became as important in the eighteenth century as the intendants whom they served. I particularly enjoyed the essay on the trial of La Chalotais, the culmination of a crisis pitting the accused, the procureur general of the *parlement* of Rennes, and four other magistrates against the provincial governor and the king on the issue of fiscal reform. In his concluding essay on the future of administrative history, Antoine is optimistic. He sees an upswing during the last decade after years of disinterest and suggests avenues of future research. He makes the observation that social history is inseparable from institutional history (p. 324). A study of the personnel staffing institutions is simultaneously a study of their social, economic, and intellectual milieux and cannot be separated from a study of the institutions themselves.

This is not an important book. All the essays have appeared elsewhere, and generally what Antoine has to say is not new in the sense of challenging established interpretations. But it is not meant to be a major study; it is a collection of essays. It is a book for specialists, particularly noninstitutional historians who would like to gain familiarity with administrative history as it is practiced today by an acknowledged master of the art. Antoine's work is characterized by meticulous attention to the documents that are his evidence. He weaves them generously into the text, and they appear as *pièces justificatives* at the end of individual essays. They give the book a tone of authenticity that is unmistakable and a hallmark of both Antoine's work and administrative history. Despite large chunks of Old Regime French, the book is surprisingly easy reading because of its style, which is classic in its simplicity. The book is rigorously reasoned, carefully constructed, sparse in detail; each word is the right one. It has a linear elegance that is a hallmark of Antoine's work. I am a great admirer of his style, which reminds me of the work of Gustave Dupont-Ferrier in its clarity of vision and expression. I only wish that in this particular case his superb technique could have been exercised in a weightier study. But the essays shine for their style, for the complex made deceivingly simple. This is not a book for the general reader, but it is a book for specialists who would like to see how a master does administrative history.

SHARON KETTERING
Montgomery College

GAY L. GULLICKSON. *Spinners and Weavers of Auffay: Rural Industry and the Sexual Division of Labor in a French Village, 1750–1850*. New York: Cambridge University Press. 1986. Pp. x, 256. $34.50.

A fine example of meticulous scholarship, Gay L. Gullickson's valuable book is a study of proto-industrialization in Auffay, a village in the Pays de Caux of Normandy over 100 years. The volume is set up as a classic social history emphasizing relationships between economic and demographic change, but with a difference—a thoroughgoing analysis of the sexual division of labor in the households of Auffay.

Gullickson opens with an examination of causes and consequences of proto-industrialization as they have been sketched out by Franklin Mendels and others. Mendels defines proto-industrialization as a distinct stage in European economic development in which large-scale cottage industry was typical; merchants put work out into the countryside and collected it to be sold not locally but in distant (national and international) markets. Mendel's "model" posits that proto-industries were commonly found in areas of subsistence or pastoral farming with large populations accessible to urban cloth merchants. The consequences most often discussed are demographic: whole families were drawn into proto-industrial work, age of marriage declined, more children were born to couples because of early marriage. This meant rapid population growth and family and regional crises when the industry contracted. Gullickson finds typical proto-industrial production in the Pays de Caux but under different circumstances, namely high levels of popular landlessness in a commercial farming region.

Her sources include parish records, from which she constructs a family reconstitution, census nominal lists, tax rolls, government reports, and a few memoirs and folkloric accounts. With this social historian's kit bag, Gullickson describes three periods over the century. The first is the "golden age of spinning" (1750–86). A ten-to-one ratio of spinners to weavers was required to supply weavers the necessary cotton thread to do their job; hence, a large demand existed for cotton spinners. They were mostly women, whereas weavers at this time were all male. This division of labor was possible because the commercial agriculture of the region had uneven demand for labor; the harvest offered work to every able-bodied hand, including women, in the region for some weeks. A much smaller male year-round labor force was needed for maintaining the large farms. Spinning satisfied women's need to work very well, for it was possible to integrate spinning with housework and child care.

The second period followed the Eden Treaty with England (1786), which permitted English machine-spun cotton fabrics to be imported and sold in France. The consequence was unemployment and hardship in the Caux as English competition put French spinners and weavers out of work just as the political crisis came to a head in Paris. Waves of food

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protest spread across the region in summer 1789. By 1790 and 1791 the cotton industry began to mechanize, first through spinners using jennies in their own cottages. In the 1790s spinning mills were introduced by manufacturers with capital. Jobs in these mills were insecure, for the mills laid off workers when there was a slump in demand, but cottage weaving was no longer viable work. As the yarn supply from mechanized spinning became reliable in the first decade of the new century, women were offered weaving work, until then a job reserved for men.

The third period, starting with the Restoration, was a "golden age of cottage weaving" in which women became part of that labor force as weavers of lightweight calicoes. A division of labor according to fabric produced—men wove heavier weights—made it possible to pay women less for their time than men. Women also moved into sewing and agriculture on a year-round basis; the agricultural revolution had brought about new mixtures of crops and rotations that required year-round workers. This period ended in the 1840s and 1850s with the mechanization of weaving and massive outmigration from the Caux to cities.

This tripartite discussion of the history of proto-industrialization in the Caux is followed by chapters on marriage and childbearing, widowhood and remarriage, and illegitimacy.

Gullickson then returns to proto-industrialization and shows that the demographic effects felt in the Caux were quite different from those found in earlier studies of proto-industrialization. The age of marriage did not drop; rapid population growth did not occur. This is because, she concludes, proto-industrial work in the Caux primarily involved women, rather than men or whole families as elsewhere.

Although her polemic on proto-industrialization sometimes seems a bit repetitious and heavy-handed, Gullickson has written an excellent book that makes its points effectively because of the ingenious use of relatively limited sources and good writing.

LOUISE TILLY
New School for Social Research

RAN HALEVI. *Les loges maçonniques dans la France d'Ancien Régime: Aux origines de la sociabilité démocratique*. (Cahiers des Annales, number 40.) Paris: Armand Colin. 1984. Pp. 118. 56 fr.

The more than eight hundred Freemason lodges in eighteenth-century France were the most extensive, best-organized, and best-documented of the free associations (the so-called secret societies) that sprang up at the end of the Old Regime. Whether they effected and reflected a "mutation de sociabilité" (Maurice Agulhon's phrase for the emergence of democratic assumptions) is in dispute. Responding to Gerard Gayot's view of freemasonry as interwoven with corporate society, Ran Halévi sees the lodges as fostering prepolitical democratic sociability and internally establishing a new style of social organization that their members came to believe should replace corporatism throughout political society. Using the archives of the Grande-Loge and the Grand-Orient in the Bibliothèque nationale, Halévi finds that the administrative reform of 1773 marks the change from an association in which equality was merely an animating ideal to one that applied this principle in its own practice.

From the same sources, Halévi also traces the chronology and geography of French freemasonry, putting together the first precise measure of its dispersion through France between 1725 and 1789. The patterns of dissemination he finds open up a new area of investigation, for they do not replicate any of the major previously charted sociocultural evolutions of the century: trends in literacy, schooling, the circulation of books, the spread of academies, urbanization, population growth, or prosperity. That the founding of Freemason lodges is largely autonomous of these trends suggests that historiography to date has erred in viewing the movement merely as an aspect of the Enlightenment or of the prerevolution.

Thus, although Halévi offers only a brief setting out of the issues rather than an exhaustive treatment of them, he opens the way to a fresh sociological and intellectual study of freemasonry on its own terms, as a vision of the world that appealed to, and shaped the understanding of, the last generations of Old Regime France.

CAROLYN C. LOUGEE
Stanford University

ROBERT D. HARRIS. *Necker and the Revolution of 1789*. Lanham, Md.: University Press of America. 1986. Pp. ix, 805. $38.75.

This is the second of a projected three-volume study of the Genevan banker and statesman Jacques Necker (1732–1804). Robert D. Harris's *Necker, Reform Statesman of the Ancien Régime* (*AHR* 85 [1980]: 638–39) is one of several distinguished books written during the past fifteen years that have fundamentally revised the record of Necker's political achievement before 1789. The present study recounts the story of his second and third ministries (August 1788 to September 1790).

Necker came to office after the dismal failure of Charles-Alexandre de Calonne and Etienne-Charles de Loménie de Brienne, when the government was

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on the edge of bankruptcy, and an explosive artistocratic reaction had roused the nation to a fervid pitch of political protest centered on the States General whose convocation King Louis XVI had just ordered. Necker arrived with an enormous reputation as a progressive finance minister and the author of the widely read *Compte Rendu* (1781) and *Administration of Finances* (1785). "Neckerist" ideas of government retrenchment, curtailing court expenditures, public administration of national finances, rationalizing and decentralizing taxation, and publishing government receipts and expenses won the minister a huge following. Then, in the aftermath of the Royal Session, the ascendancy of the ultraroyalist party, and the calling of armed troops to Paris, the king proceeded to sack Necker on the eve of the fall of the Bastille. Later that month Necker was recalled amid popular jubilation as savior and martyr of royal despotism. Despite the Genevan's triumphant return, his popularity and influence waned quickly. He resigned in 1790 after all his initiatives were either overruled or ignored by the Constituent Assembly.

How can one explain the dramatic reversal of a man who only two years before had been the nearly universal choice as finance minister? Admittedly, Necker had many enemies: spendthrift courtiers, absolute monarchists, jealous politicians, disgruntled financiers, and radical leaders of the Third Estate. Yet many unbiased contemporaries who admired Necker personally and who sympathized with his moderate views—Arthur Young, Thomas Jefferson (American ambassador), Jean-Sylvain Bailly (president of the National Assembly), and constitutional monarchists such as Jean-Joseph Mounier and Pierre-Victor Malouet—found Necker lacking in political judgment, decisiveness, and leadership ability—someone who could not translate probity, brilliance, political balance, genuinely humane sympathies, a taste for compromise, and an invincible love for king and country into a viable political program. Historians of such distinction as Marcel Marion and Georges Lefebvre have echoed these sentiments in our own day. No partisan of the calling of the States General, Necker had reservations about the prudence of doubling the *tiers*, the union of the three estates, and the fate of the executive authority at the hands of the people's representatives. Unable to mediate the rival claims of the privileged classes and the Third Estate and incapable of opting for the nation over his beloved sovereign, Necker found himself rejected by all parties.

Harris, although disavowing any motive of apology or rehabilitation, is still profoundly admiring of Necker, whom he extols as a judicious, liberal, but misunderstood statesman, caught up in the toils of intractable revolutionary problems and suffering the calumnies of extremists on the Right and the Left. Harris's aim is to give the minister a fair hearing, to judge contemporary issues as Necker experienced them. For the most part the author has succeeded. The book is a searching and exhaustive analysis of Necker in power. One may inquire if Harris would not have written a stronger, clearer, more emphatic book if he had compressed its 804 pages to the size of his first volume (260 pages), the same space Harris now allots to Necker before his accession to power. There are chapters describing the "tortuous labyrinths of royal finances" and "battle of figures" between Necker and his detractors. Much space is devoted to Necker's noble intentions, his devotion to virtue, reason, and justice, his fealty to the social hierarchy, his unswerving loyalty to the king, his admiration of the British constitution. Harris's Necker is an honest and fair-minded public servant who shrank from opportunism, deceit, and class conflict but who ultimately failed both to convince Louis XVI that he should be a king of England and to translate liberal doctrine into a reform program that won the support of the French.

Harris has written a careful, painstaking, and on the whole balanced portrait, resting on an exhaustive repertory of contemporary sources. The focus is nearly exclusively economic and political, with a smattering of Necker's religious philosophy and occasional glimpses of the minister's famous daughter, Mme. de Stael. In the third volume Harris promises to conclude his examination of Necker in the French revolution and to render an account of Necker's political career. Historians and students will be in his debt for the completion of this authoritative work.

LOUIS S. GREENBAUM
University of Massachusetts

R. R. PALMER, editor and translator. *The Two Tocquevilles, Father and Son: Hervé and Alexis de Tocqueville on the Coming of the French Revolution.* Princeton: Princeton University Press. 1987. Pp. viii, 252. $28.50.

The imminent bicentennial of the French revolution invites and obliges historians to consider anew the historiography of this world historical event and *longue durée* process. This task is discerningly furthered by R. R. Palmer's *The Two Tocquevilles.* He edits, translates, and comparatively analyzes seven chapters from Hervé de Tocqueville's *Survey of the Reign of Louis XVI* of 1850 and seven chapters, fragmentary drafts, and notes of Alexis de Tocqueville's never completed work on the revolution and Napoleon. To this he adds excerpts from Alexis de Tocqueville's correspondence with friends about his project. The subject of both historians is

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the fatal crisis confronting the monarchy in the years 1787 to 1789 culminating in the calling of the Estates General and the formation of the National Assembly.

Both father and son used such valuable sources as P. J. B. Buchez and P. C. Roux's *Histoire parlementaire de la Révolution française* (1834–38) and the pamphlet literature attending the revolution. Palmer emphasizes that Hervé's history was much indebted to Joseph Droz's *Histoire du règne de Louis XVI pendant les années ou l'on pouvait prevenir ou diriger la Révolution française* (1839). Consequently, it was not received as an original or important history. But the value of Palmer's consideration of Hervé's book is highlighted by his unexpected judgment that "Hervé's account of the three years preceding July 1789 closely resembles the treatment by Georges Lefebvre in his *Coming of the French Revolution*" and also the direction taken on the advent of the revolution in the works of Jean Egret, François Furet, Daniel Richet, and Michel Vovelle (p. 20, n. 27). This observation is certain to stimulate the coming reconsideration of the revolution's historiography.

Hervé de Tocqueville was essentially, as Palmer proposes, a narrative historian, and Alexis was guided by an analytical intent and practice. Yet it is interesting to see father and son alike accenting the importance of class, the contradictions and dilemmas overwhelming the participants facing the storm, and the changing spirit of public discourse, language, rhetoric, and representation advancing the revolution. Hérve's programatic education, as a prefect for thirteen years during the Restoration, is everywhere present in his account of the failures in crisis management by those who served Louis XVI.

Alexis's oratorical education, as a member of the Chamber of Deputies from March 1839 to December 2, 1851, is similarly evident in his consideration of the revolution as a crisis of culture and civilization. The signifying intensity of the forms of revolutionary rhetoric and the force of its representations and codes were central to his analysis of the revolution as a text demanding profound and universal interpretation. His frequent use of illness as a political and historical metaphor is always complemented by his references to the "moral grandeur" of the revolution (p. 224). For Alexis de Tocqueville, as for Jules Michelet and Jean Jaurès, the interesting questions were those of *la morale*. This priority will also merit attention in the coming fetes celebrating the revolution's anniversary.

EDWARD T. GARGAN
University of Wisconsin

DONALD E. ENGLISH. *Political Uses of Photography in the Third French Republic, 1871–1914*. (Studies in Photography, number 3.) Ann Arbor, Mich.: UMI Research Press. 1984. Pp. xvi, 262. $42.95.

Donald E. English sets out to discuss the role of a popular nineteenth-century invention that broadened the use of pictures from traditional images (such as woodcuts) and even lithography, invented in 1798, to a mass product that proved politically effective in the new age of mass politics.

The Second Empire had seen photography advance from nonreproducible daguerrotypes to new products capable of begetting innumerable copies. Then, beginning in the 1880s, new technological breakthroughs provided smaller, portable, handheld cameras, clearer images on film, and the photomechanical halftone process that made possible the mass production of photographs on posters, leaflets, above all in newspapers and reviews.

The author's introduction offers a swift and meaty survey of what went on in many areas: portrait photography, military reconnaissance work and map making, microfilming of messages (used in conjunction with pigeon and balloon post during the siege of Paris), pornography, the arts, and anthropometry (treated more fully in chapter 2). The rest of the book focuses on photographs and photojournalism as used by the political Right and on—mostly official—attempts to censor or counter the new techniques of political manipulation. This is discussed in chapters on the Commune and its aftermath, then on Bonapartist, Boulangist, royalist, anti-Dreyfusard, and anti-Semitic propaganda.

The theme is fascinating, the treatment becomes progressively less so. Like contemporary businessmen, scientists, and artists, *fin-de-siècle* politicians realized that the new technology could be useful, and they soon put it to use. Once we have learned this, the account of what was done tends toward repetition and toward *déja-vu*. The author's interpretation of what went on is not always convincing, nor does he suggest much that is new about France in the two score years preceding 1914, unless it be French resourcefulness in finding new ways to beat each other over the head. Ouriel Reshef's *Guerre, mythes et caricature* (1984) provides rather more sophisticated discussion of similar material. Reshef's psychohistorical demonstrations do not compel belief, but he leaves a more nuanced and diverse impression of graphic enterprise during those busy times. In the present book more comparative reference to competing political activities would have helped. It is hard to gauge the relative effectiveness of the maneuvers English describes without some reference to rival enterprises—especially those of less marginal groups than those he deals with.

None of this means the book is useless. The knowing reader will find much suggestive material in it, notably sixty-nine plates that could be better

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reproduced but that are full of interest, especially when English describes and illustrates early and ingenious use of photofakery, media manipulation, and the creation of media events attractive to photographers.

The book reads like a revised dissertation, but dissertations have their uses. The patient compilation of detail is a virtue too, especially when its results suggest the exciting possibilities of the materials plumbed.

EUGEN WEBER
University of California, Los Angeles

PEGGY A. PHILLIPS. *Modern France: Theories and Realities of Urban Planning.* Lanham, Md.: University Press of America. 1987. Pp. ix, 252. Cloth $26.50, paper $14.50.

Peggy A. Phillips has written a book about the growth of urban planning in contemporary France and its links with territorial and economic planning. In a complex political and administrative process that she traces back to the 1880s, the main theme is the growth of "corporatism." Phillips defines this not as fascist corporatism but as a welfare market economy based on a social compact and administered by technocrats. The best part of the book deals with the creation of a French national planning system since 1945 and its political implications. Phillips is less successful in her attempt to link national to urban planning on the lines sketched out in her assertion, "National economic planning and concomitantly fiscal policy from the beginning rested upon national, or global, level urban planning" (p. 66).

For Phillips urban planning began in the early Third Republic with a public housing program (habitations à bon marché [HBM]). This allows her to make some interesting reflections on Radical and Socialist municipal politics, but in the absence of any measurement of the volume of public housing built before 1914 we are unable to assess its significance. Frequently, Phillips replaces specific factual information and empirical analysis by ideal forms, concepts, and paradigms similar to the "corporatism" that she uses in her political analysis. Already before 1914 Ebenezer Howard and his "garden city" have been joined by Le Corbusier and his "vertical garden city," and in the twentieth century Phillips increasingly uses Le Corbusier and the "grandes [*sic*] ensembles" as symbols of an environment that is perceived as stretching across the whole of urban France. To have any chance of working, this technique would require an accurate knowledge of Howard, Le Corbusier, and their work, but Phillips is glaringly deficient in this respect. Only in her discussion of Henri Sellier's work in interwar Paris does she make the desired connection between city building and regional and national politics.

Phillips's discussion of urban planning issues is marked by unusual judgments and flagrant errors, and detection of their source is complicated by her practice of using compound footnotes for whole paragraphs or even groups of pages, usually without precise page references. For instance, Le Corbusier never advocated "cities within single buildings" (pp. 4–5). He designed none of the *grands ensembles* and accepted no responsibility for them. Howard did not supervise the creation of the factory village of "Bourneville" (p. 20). On the same page the title of Howard's pioneering book of 1898 is miscited.

Such defects drive the reader back to the discussion of national planning, which, as a narrative, functions adequately, albeit in an area that has been much discussed. Consequently, the book will be of some value to students of post-1945 France.

ANTHONY SUTCLIFFE
University of Sheffield

SHERRIN MARSHALL. *The Dutch Gentry, 1500–1650: Family, Faith, and Fortune.* (Contributions in Family Studies, number 11.) Westport, Conn.: Greenwood. 1987. Pp. xxvii, 225. $35.00.

The Dutch gentry were part of a legally defined noble estate. Their numbers were declining, particularly during the seventeenth century. Many never married, and birth rates were lower than among the general population. Whereas the "high" nobles controlled important national offices, the gentry occupied positions of middling rank and dominated the provincial estates. The Dutch gentry thus had a social and economic situation similar to that of their English counterparts, but their role in national politics was more limited. They constituted a pivotal group between the greater nobles, the towns (into whose patriciates they frequently married), and the peasantry. The gentry of Utrecht as constituted in 1536 are Sherrin Marshall's paradigm. She traces their kin and patron-client relationships back to 1500 and forward to 1650, as they were extended through the other provinces of the United Netherlands.

The author's central themes are that the study of the family provides insights into more obviously public areas of human conduct and that family relationships were governed by a principle of reciprocity tempered by clear lines of authority. As a group, the gentry of Utrecht held their own economically, and Marshall wisely refuses to speak of rise or fall. The impact of the revolt for Dutch independence on them has been overestimated. Death rates were predictably high, particularly for males, as was the number of remarriages as a

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percentage of total marriages. But, while individual gentry played an important and sometimes fatal political role, most families survived the crisis.

Just as God entered engagements toward his creatures, human beings, so husband and wife, and parents and children, had duties of love, support, and in some cases obedience toward one another. Although "romantic love" was not a precondition of marriage, it did exist, and the couple's wishes were normally taken into consideration by their parents. In law the wife was clearly subordinated to the husband, but this was mitigated considerably in practice. Although there was a considerable disparity in age between spouses at first marriage in the early sixteenth century, a decline in male life expectancy and rise of female meant that couples were more often of comparable age by 1650. Marshall rightly rejects the notions that the lineage controlled the "core family unit" and that the family was essentially devoid of affective relationships, but this was not a recent development in the sixteenth century. Virtually all considered religion important, but most, both Calvinists and Roman Catholics, were rather tolerant, and Marshall sees no clearly defined ideology or religious belief permeating the gentry. Although her demographic data are statistically based, she prefers in discussing the emotional or qualitative aspects of family life to let the gentry speak for themselves through their correspondence, wills, and marriage contracts.

This is an intelligently argued book, based on an exhaustive and clearly presented use of printed and archival materials, citing and generally supporting other recent scholarship on the early modern gentry. My reservations are quibbles. Marshall's concluding chapter, on the political role of the gentry in the revolt against Spain, is the only one whose conclusions seem restricted largely to Utrecht, and it is not well integrated into the more generally applicable social observations of the rest of the book. Although I agree that family relationships were based on reciprocity and that women and children seem to have been dominated less by patriarchy and lineage in the Low Countries than elsewhere, I am less convinced by her attempt to use reciprocity as a generally applicable principle of social and even political intercourse. Finally, some of the bar graphs are printed in an illegible form in the review copy, forcing the reader to rely on the author's explanations.

DAVID NICHOLAS
University of Nebraska

GUNNAR ARTEUS. *Till Militärstatens Förhistoria: Krig, professionalisering och social förändring under Vasasönernas regering* [The Prehistory of the Military State: War, Professionalization, and Social Change under the Vasas]. (Militärhistoriska Studier; Utgivna av Militärhistoriska avdelningen vid Militärhögskolan, number 8.) Stockholm: Probus. 1986. Pp. 201.

As the very title of this work implies, it is accepted as established that by the high point of its "imperial" experience (the mid-seventeenth and early eighteenth centuries) Sweden had developed a "military state." In this book Gunnar Arteus demonstrates that the condition, or at least important aspects of it, was already developing vigorously in the second half of the sixteenth century. He respectfully challenges elements of the Gustavus Adolphus cult and the eminent historians (such as Michael Roberts) who have most recently contributed views on the military significance of that great warrior king. Carefully denying that Swedish society as a whole or even the nobility was being "militarized," he does prove (with statistics and appropriate apologies for their fragility) that a significant number of lower- and middle-rank military officers serving the three Vasa kings of the period 1555–1610 had become at least measurably "professional." They served for long periods, received regular pay and pensions of some sort, rose through established ranks, served in peace as well as war, accumulated great practical experience, developed military attitudes, and even achieved a sort of corporate status. The lengthy wars of the period, the heavy use of foreign professionals, and the expansion of royal power contributed to this development, even to some extent overriding the religious priorities of the time.

Arteus makes quite clear that, in contrast to the full-blown professionalism and militarization that came later, the sixteenth-century development was limited. Nobility was still a more important prerequisite for appointment to high command than professionalism as defined by the author. The cultivation of theoretical studies was limited or lacking. Many careers were of mixed military-civilian activity. Military service, although respected, did not yet confer on the officers any notable status or privilege.

The biographical data available simply do not provide us with the real reasons why officers were appointed, promoted, assigned, dismissed. Arteus's hard evidence can only provide him (and us) with a skeleton. Fortunately, he pushes past his methodological quiverings to flesh out his story with educated guesses (clearly labelled). Over fifty pages with names and origins of identified officers will be useful in further research. A very decent ten-page summary in English and a dozen tables in that language make the main contents (though not the nuances) available to scholars who do not read Swedish.

HEINZ E. ELLERSIECK
California Institute of Technology

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ULRICH LANGE. *Landtag und Ausschuss: Zum Problem der Handlungsfähigkeit landständischer Versammlungen im Zeitalter der Entstehung des frühmodernen Staates; Die welfischen Territorien als Beispiel (1500–1629).* (Veröffentlichungen der Historischen Kommission für Niedersachsen, number 24; Untersuchungen zur Ständegeschichte Niedersachsens, number 6.) Hildesheim: August Lax. 1986. Pp. 278.

Early modern European history is characterized by the growth of centralized and absolute governments. The evolution in Germany, though bearing resemblances to what occurred in Western Europe, has, however, its own idiosyncratic features. There the development of strong government took place not in the empire but in the individual territories. Yet the rulers of these territories had to contend for power with their local estates, represented in the diets. These bodies had a long history, even outside the borders of the empire, going back to the later Middle Ages. German historians have long occupied themselves with the phenomenon of the estates and their role in government, especially as organs for granting and levying taxes.

More recently, interest has shifted away from the territorial diets themselves to the executive committees or commissions (*Ausschüsse*) of the diet. These committees usually consisted of less than ten men appointed by the ruler from names submitted by the estates. They had the advantages of being less expensive than full diets and of being more suitable to discuss confidential subjects. Because the committees met more frequently, their members acquired more experience, particularly in managing the growing complexity of the fiscal administration. Thus, over time their task was turned into one of an administrative nature. But this change also endangered the power of the estates. The *Ausschüsse* could only take over the functions of a full diet when they possessed the full confidence of all the estates. But, the more time they tended to spend in the proximity of the prince and his councilors, the more they became alienated from the estates, from which they drew their legitimacy. Once that happened, they were no longer credible representatives of the estates to the ruler. During the seventeenth century the *Ausschüsse* were transformed into close oligarchies, often bound together by family alliances. At that point they had become organs of the newly centralized power.

Ulrich Lange examines this process in the three Brunswick territories ruled by the house of Welf in the period between 1570 and 1629. There is a time lag between what occurred there and in southern Germany. Meetings of the estates did not become institutionalized in the territories of Lüneburg, Wolfenbüttel, and Calenberg-Göttingen until the sixteenth century, whereas the system of executive committees only arose in the 1590s, when the burden of the princely debt and, with it, the increasing demand for taxes enabled the estates to step in. Otherwise the Brunswick experience appears to fit the general paradigm.

The work, cast in the form of a research report typical of German historiography, is based on considerable painstaking archival research. The task was complicated by the fact that much valuable material was destroyed in 1943. Fortunately, the damage was largely confined to the archives for Lüneburg; those of Calenberg and Wolfenbüttel are in reasonably good condition. Lange's book, therefore, is an important, if only partial, contribution in tracing the constitutional evolution in the German territorial states from the end of the Middle Ages through the Thirty Years' War. Together with other such studies on different territories, some already published, it will help us round out the picture.

HANNS GROSS
Loyola University
Chicago, Illinois

ALOYS WINTERLING. *Der Hof der Kurfürsten von Köln 1688–1794: Eine Fallstudie zur Bedeutung "absolutischer" Hofhaltung.* (Veröffentlichungen des Historischen Vereins für den Niederrhein insbesondere das Alte Erzbistum Köln, number 15.) Bonn: Ludwig Röhrscheid. 1986. Pp. ix, 282.

Aloys Winterling analyzes one of the most visible aspects, and perhaps one of the least understood, of late European absolutism: the court and its aura of etiquette and ceremony. Choosing the Electorate of Cologne for his case study, he has produced a book that does precisely what a good monograph is supposed to do. It is based solidly on primary sources and archival materials, it covers its own subject area thoroughly and carefully, and it suggests linkages and comparisons with the wider world of German and European absolutism. The text is well written and clear, the analysis perceptively done. Tracing the evolution of the court in terms of the persons who composed it from the late seventeenth century, he shows how the local nobility—landholding aristocrats and members of the cathedral chapter of the Archbishopric of Cologne—used the court to advance their own interests and came to monopolize the chief offices of government and court by the end of the eighteenth century. He contrasts this with France under Louis XIV, where the court system enabled the king to attract, supervise, and seduce his nobility and render them from malcontents into courtiers. In Cologne the court and its social life not only failed to reinforce absolutism but actually furthered its decline. In addition, the enormous expenditures on palaces, balls, and hunting expeditions

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took away funds that might have been used to build a standing army to intimidate the estates. Winterling suggests that an explanation of such expenditures by the electors makes sense only when they are considered in a supraterritorial role, as princes of the Holy Roman Empire. The study offers a good example of how court traditions and ceremonies modeled on French practice came to have a very different and very native role in Germany.

The study is enhanced by a large appendix of eighteen contemporary documents and fourteen illustrations, which support arguments in the text about court life. This is a competent limited study that is suggestive of further work and contributes to our knowledge of eighteenth-century German government and society.

ROGER WINES
Fordham University

SISKO HAIKALA. *"Britische Freiheit" und das Englandbild in der öffentlichen deutschen Diskussion im ausgehenden 18. Jahrhundert.* (Studia Historica Jyväskyläensia, number 32.) Jyväskylä: Jyväskylän Yliopisto. 1985. Pp. 346.

Much has been written about the intensity and variety of French interest in Great Britain in the eighteenth century. The Anglophobia generated by imperial competition was counterbalanced by admiration for British toleration, as expressed by Voltaire, and for British recreations, as expressed by the Duke of Orléans. Less familiar is the attitude of educated Germans, so a corresponding welcome should be given to Sisko Haikala's careful and comprehensive survey. Because he has worked through eighty-one periodicals and one hundred contemporary pamphlets and other single works, he is able to write with considerable authority about how Germans were influenced by what they believed to be happening in the British Isles.

Perhaps his most interesting finding is just how extensive cultural contacts between the two countries were. By the early eighteenth century, the English language was being taught as an academic subject at the Protestant universities of northern Germany, spreading even to the Catholic universities of the south by the 1780s. As a consequence, British (a word chosen advisedly, for the works of the Scottish Enlightenment were among the most influential) literature, newspapers, and periodicals enjoyed a large and constantly growing readership. By the end of the century there were six German Protestant churches in London and even a German club for the large colony of expatriates (although the figure of three hundred thousand cited must surely be a misprint, since it seems improbable that every third Londoner was a German).

More predictable is the way in which German attitudes developed in the course of the century. Until the 1770s Great Britain was seen as the epitome of political, cultural, and social liberty. Seriously damaged by the American war, that enviable reputation was challenged more fundamentally by the alternative model of progress offered by the French revolutionaries. Fortunately, Haikala does not try to conceal the pleasing lack of a unified response, even among the more progressive sections of the German intelligentsia. If there was a bear market in British stock after 1776, and if it plunged still further in the heady days of 1789, when so many rushed to transfer their funds to France, the revolution's degeneration into anarchy, violence, and deprivation swiftly discredited the new company's prospectus and brought many an investor creeping shamefacedly back to the old firm. Some had never left, whereas others rejected all varieties of liberty, preferring the apparently more gilt-edged stock of traditional absolutism. Indeed, it is gratifying to discover that Anglophobia came both from the far Left and the far Right, as they combined to see Great Britain as the embodiment of luxury, materialism, hedonism, degeneracy, inequality of wealth, arrogance, and ambition. These perceptive Germans also regarded the British—especially the English—as peculiarly prone to violent crime, cruelty, superstition, eccentricity, and suicide (among many other things).

If the pace of this book is slow and carefully measured and if some of the terrain proves heavy going (notably the lengthy and unexciting exposition of the familiar views of Ernst Brandes, August Wilhelm Rehberg, and Friedrich Gentz), the patient traveler will find the journey worthwhile in the end.

T. C. W. BLANNING
Sidney Sussex College
University of Cambridge

FERDINAND MAGEN. *Protestantische Kirche und Politik in Bayern: Möglichkeiten und Grenzen in der Zeit von Revolution und Reaktion 1848–1859.* (Kölner Veröffentlichungen zur Religionsgeschichte, number 11.) Cologne: Böhlau. 1986. Pp. x, 391. DM 98.

Of all the nineteenth-century German states, Bavaria was the only one in which a minority Protestant population faced a Roman Catholic ruling house. Yet even to speak of a Bavarian "Protestantism," is incorrect, since there were substantial differences among the kingdom's Protestants, above all between the "Union" church of the Palatinate (Bavaria's province on the left bank of the Rhine), formed in 1818 by a merger of Calvinists and Lutherans, and the more strictly Lutheran church in

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Franconia and Swabia. Ferdinand Magen sets out to investigate the implications of this divided, minority position for the structure of Protestant church institutions and their relationship to the state, for the church's teachings about politics, and for the participation of church representatives in political life from the revolution of 1848 through the reaction era of the 1850s and on to the beginnings of a liberal resurgence at the end of the decade.

The author describes in great detail the organizational structures of the church before 1848, outlines their changes during the revolution (few, and most of these quickly revoked), and discusses their further development during the age of reaction. He elucidates the organization of charitable activities and religious revivals and explores the attitudes of leading churchmen toward issues such as education, marriage and the family, the "social question," and relations to other confessions. Finally, Magen concludes with a consideration of the relationship of controversies within the church to political conflicts in the late 1850s.

The author does not do a particularly inspired job with his materials. His style is, at best, tedious, the threads of his argument are not well connected, his conclusions are meager. Much of the study consists of a recapitulation of the details of institutional development, available elsewhere in published form. As far as I can see, the general trend in his work is to support well-established theses: theological conservatives were political conservatives, whereas theological liberals were political liberals; Bavaria's conservative Lutherans were suspicious of synods and lay influence, preferring instead to maintain the power of the prince—even a Roman Catholic one—as *summus episcopus*; mid-nineteenth-century Protestants viewed the social question largely in terms of the moral improvement of the lower classes. Magen's occasional attempts to play up individual exceptions to these general trends are unconvincing, his sniping at other authors, such as Werner Blessing, who have dealt with these topics, is not particularly helpful.

In spite of the author's proclaimed comparative perspective, he provides little comparison with the Protestant churches in other German states or with the position of the Catholic church in the predominantly Protestant German states. (It would seem that Catholics in, say, Prussia were much more successful in using the events of 1848 to gain a greater measure of independence from state control than Bavarian Protestants were or perhaps wanted to be.) Finally, he has relatively little to say about the political activities of individual pastors or active lay people at the local level, although that is probably where the church's influence on politics could be felt most strongly. All in all, I found this a dull and disappointing work.

JONATHAN SPERBER
University of Missouri, Columbia

SUSANNE MILLER and HEINRICH POTTHOFF. *A History of German Social Democracy from 1848 to the Present.* Translated by J. A. UNDERWOOD. New York: Berg; distributed by St. Martin's, New York. 1986. Pp. xii, 330. $35.00.

This is a translation of the 1983 edition of the much reprinted original, *Kleine Geschichte der SPD: Darstellungen und Dokumentation 1848–1980* (first published in 1981). The German title is more accurate in three ways: in suggesting that it is a brief history, in confining itself to the Social Democratic party (SPD) proper, and in referring to documentation. On this last point, although the English edition contains only half the documents of the original, it still has nearly ninety pages of tables and documents and just over two hundred thirty pages of text.

Although an adequate and even insightful account of the history of the party, Susanne Miller and Heinrich Potthoff's book is certainly not a history of the German social democratic movement—the sweeping insurance, cultural, artistic, intellectual, and sports and recreation activities sponsored by and affiliated with the party during its long history. Rather, the authors concentrate heavily on the political history of the party and its political successes and failures, the failures being rather more numerous than the successes.

Even the more limited promise of the original German title could not be adequately fulfilled in so short a text. Many events that were extremely complicated are of necessity dealt with briefly. For German readers with a strong background in SPD history, this is not a serious weakness, but American and English readers lacking this background will often find themselves at sea trying to understand just what was happening. Some examples of this problem are the discussion of the so-called *Jungen* revolt of the early 1890s (p. 47), Karl Liebknecht's opposition to war credits in 1915 (pp. 57–58), and the party crisis engendered by the Emergency Laws of 1968 (pp. 183–84).

The lack of sufficient background is compounded by a sometimes awkward translation. Converting scholarly German into elegant English is a monumental task, of course, and the translator of this volume (J. A. Underwood) has managed only a workmanlike job. In addition to some incorrect English (including reference to a "badly holed" parliamentary system on page 112, using "relativise" on page 127, and offering the phrase "indissolved

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residue" on page 193), a great many passages are so convoluted as to be very difficult to understand.

Potthoff deals with the years up to 1945, and Miller covers the period since then; the results of this divided responsibility are uneven. Miller reads more smoothly, both in this translation and in the German original. Potthoff offers no new or convincing explanation for the failure of the party to achieve its espoused goals and deals in only a cursory manner with the internal weaknesses of the SPD. Miller is more persuasive, especially when she quite rightly emphasizes that the virtual obsession of the SPD with national reunification colored and weakened nearly all of its postwar positions.

Furthermore, several opportunities for revealing comparisons across time are missed in this format. For instance, a comparison of the SPD's reaction to the *Jungen* revolt with the reaction to "the young left" at the Mannheim conference of 1975 could have revealed much about how the party had changed in the intervening eighty-five years. Similarly, a brief comparison of the SPD's response to the military build-up of the early years of this century with the party's stance on rearmament and nuclear weapons in the post-1945 era could have strengthened the sense of a party learning to accept responsibility for its size and influence.

In sum this book will be useful to experts in the field for a survey of the major features of the history of the SPD, but it is not appropriate as an introduction to the topic. Its rather narrow focus on politics leaves out many of the more interesting aspects of the movement, especially its theoretical activities.

GARY P. STEENSON
Palo Alto, California

PETER BERGMANN. *Nietzsche, "the Last Antipolitical German."* Bloomington: Indiana University Press. 1987. Pp. 239. $29.50.

This latest intellectual biography of Nietzsche takes as its point of departure Nietzsche's characterization of himself as "the last antipolitical German." His antipolitics signified not indifference to politics but rather "insistent protest of politization, of the political secularization of highest values" (p. 164). Coined in the sixteenth century by the *politiques* to denounce advocates of theocracy, the term "antipolitical" was used by Nietzsche in the affirmative sense of defending culture from the encroachment of the state. The struggle against politization formed the constant motif in a life marked by stunning apostasies. Peter Bergmann attributes Nietzsche's break with Wagner to his disenchantment with Wagner's belief that increased political power might regenerate culture.

The novelty and value of this book lie in Bergmann's painstaking examination of Nietzsche's reactions to and interaction with the political events and currents of his time. Chiding both "gentle" and "tough" Nietzscheans for situating Nietzsche above or outside of his time, Bergmann carefully traces the influence of contemporary affairs on Nietzsche's thought and work. What emerges is the portrait of an iconoclast who reflected and responded to the hopes, fears, and obsessions of his contemporaries. Bergmann posits a quasi-Freudian generational conflict: "It is only within the drama of his own age group that Nietzsche's antipolitics can be understood" (p. 55). Nietzsche is cast as the foremost intellectual representative of a "missing generation," politically and culturally overshadowed by the founders' generation (Bismarck, Wagner) and destined to be overtaken by the generation of William II before it could emerge from the shadows. The political incarnation of Nietzsche's ever more paralyzed and anonymous "generation of 1866" was the ill-fated Frederick III, whose reign lasted only ninety-nine days in 1888. Coming of political age as admirers of Bismarck in the heady decade of unification, both of these would-be generational leaders viewed the political developments of the Second Reich with growing disillusionment and exasperation. Nietzsche's preoccupation with notions of power in the 1880s reflected, according to Bergmann, his sense of personal and generational powerlessness. In 1888, the last year of his creative life, Nietzsche celebrated the liberal Frederick III as "a little shimmering light of free thought, the last hope for Germany" before the beginning of "the regiment of Stöcker" (p. 175).

Bergmann's thoughtful description of the political and intellectual context in which Nietzsche's ideas were formed offers a salutary corrective to their distortion by Nietzscheans of the Right and Nietzsche critics of the Left. Contrary to fascist interpretations (mirrored in the blanket condemnation of Nietzschean thought by such Marxist critics as György Lukács), Bergmann shows that the mature Nietzsche attacked the Bismarckian Reich not for its liberal but for its illiberal tendencies. Bergmann's analysis, however, provides little support for present-day efforts in the West to use Nietzsche as a foil against Marx. Although horrified by the Paris Commune and the rumored destruction of the Louvre, after 1875 Nietzsche increasingly disassociated himself from reactionaries manipulating the fear of socialism. His contention that socialists were inspired by resentment and revenge was provoked by Eugen Dühring's national socialism and Court Pastor Adolf Stöcker's equally bigoted brand of Christian socialism, not by the doctrines of Marx, whose works he never read. The democracy he attacked was the one that he saw embodied in the

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intolerant right-wing populism of the Second Reich. Read against this background, his elitist strictures appear as indictments more of the religious and nationalist Right than of the socialist Left. But Bergmann rightly faults Nietzsche for his total neglect of economics. His failure to ground his precepts in material social analysis rendered his antipolitics fatefully ambiguous and potentially destructive.

RODERICK STACKELBERG
Gonzaga University

HENNING KÖHLER. *Adenauer und die rheinische Republik: Der erste Anlauf 1918–1924*. Opladen: Westdeutscher. 1986. Pp. 287. DM 38.

People may be upset by this book. Henning Köhler takes on in turn Hans-Peter Schwarz, Karl Dietrich Erdmann, Rudolf Morsey, and Jacques Bariéty, as well as a number of lesser lights. Schwarz, he suggests, has advanced a picture of Konrad Adenauer that amounts to "visionary transfiguration" (p. 11). Erdmann's interpretation that at "the high point of Adenauer's particular politics" he promoted views "concerning German-French relations that only developed in the course of the decades after 1945" Köhler rejects as myth making (p. 257). Morsey and Erdmann are both faulted because "they neglect or push to the side the centrally important source of the *Kölnische Volkszeitung*" (p. 21). Concerning Bariéty, "It is hardly imaginable, but still a fact, that he failed to use the files 'Rive Gauche' in the Quai d'Orsay, critically important for the Rhine policies, but rather only the few 'captured pieces' that are translated into German and are found in the Bundesarchiv in Koblenz" (p. 138).

Köhler offers an intensive examination of the documentary evidence—including French files that were unavailable to Erdmann—on two brief periods: November 1918 to June 1919 and January 1923 to January 1924. A very short chapter on 1920 to 1922 separates the two focal points. In each period, Köhler features the role of Adenauer in the complex nexus of attitudes and negotiations relating to the Rhineland.

As lord mayor of Cologne, Adenauer clearly held a prominent position among the political elite that faced these turbulent issues. What role did he play? Which possibilities did he support or consider?

Köhler, who has written several earlier works on the Rhineland, rejects the interpretation advanced by Erdmann and extended by Schwarz that Adenauer displayed those same qualities of farsighted German patriotism and leadership that characterized his chancellorship after 1949. Köhler sees a more differentiated picture of Adenauer's political action that is not reducible to one common denominator. Köhler clearly shows that, in contacts with both the British, in whose occupation zone his city lay, and the French, Adenauer advanced ideas and schemes for the Rhineland as an independent state. He considered the region well suited by its history and character to support the peace of Europe as a buffer between France and Prussia. Further, Köhler argues that Adenauer adjusted his political line as circumstances changed. This is evident particularly once the terms of the Versailles Treaty emerged clearly, because they ended the immediate threat of annexation by France. In fact, Köhler traces six distinct phases of Adenauer's political positioning.

Adenauer's goal was a solution "that in the first place brought advantage to the Rhineland but that simultaneously contained compensations for the rest of Germany" in such matters as the payment of reparations and the occupation of the Ruhr such that "Berlin would approve and the accusation of treason would be stilled" (p. 277). The difficulty lay in the reality that no consensus could be found for such a program—not in the Reich, not in France, not even in the Rhineland. Köhler concludes that Adenauer certainly would have been judged a separatist by his contemporaries if his activities of the 1920s had been known but that these same activities show that he belonged among "those few Germans who, before 1933 saw political alternatives to the traditional national state and were willing to act on behalf of them." This disposition "allowed him after 1945 to recognize the political evolution more rapidly and to draw the corresponding political consequences" (p. 278). In other words, Köhler argues persuasively that one generation's "traitor" may well be savior to the next.

ROBERT P. GRATHWOL
Washington State University

RUDY KOSHAR. *Social Life, Local Politics, and Nazism: Marburg, 1880–1935*. Chapel Hill: University of North Carolina Press. 1986. Pp. xviii, 395. $35.00.

Beginning with the publication in 1955 of Milton S. Mayer's interviews with ten residents of Marburg who had experienced the Hitler era, the Nazi movement in this small, predominantly Protestant university town located in the notoriously anti-Semitic former Prussian province of Hesse has probably attracted more scholarly attention than has the Nazi party in any other single German community. During the past two decades, over a dozen works have appeared on the evolution of Hitler's party in Marburg and on the impact of Nazism on the town's Jews, churches, workers, and university after 1933. The present study by Rudy Koshar is the most

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recent contribution to the growing list of Marburg Nazi studies.

Unlike the detailed investigations of the Marburg National Socialist party (NSDAP) by Rosemarie Mann (1972) and John Willertz (1969), Koshar's study focuses on the social network of Marburg's voluntary organizations (*Vereine*) because "the Nazi movement was built on patterned networks of local social life" (p. 284). Nearly two-thirds of the book is devoted to a discussion of the nature of Marburg's *Vereine* between 1880 and 1930, and the balance analyzes the relationship between the Nazi party and the various bourgeois clubs. According to Koshar, Marburg's bourgeoisie used the apoliticism of voluntary clubs to "maintain its local hegemony in a contentious age of mass politics" (p. 277). By comparing Marburg Nazi party lists with the town's official registry of *Vereine*, Koshar is able to show that one-fourth of Marburg Nazis also belonged to a variety of bourgeois and student associations. These "Nazi joiners" were just as important as the Nazi activists in enabling the NSDAP by the summer of 1932 to capture the support of over half of Marburg's voters and gain control of the university's student association. Only after 1933 did the bourgeoisie fully realize that Nazi attempts to combine social and political spheres would endanger its traditional social hegemony. By 1935, concludes Koshar, the church and the university had emerged as new centers of apoliticism.

Koshar's argument that the success of National Socialism was based on a network of social and occupational associations and the support of notables can now be found in virtually all recent local and regional studies of the Nazi party. Indeed, Mann had noted that almost half of the Nazi students in Marburg in 1930 were also members of fraternities. Unfortunately, Koshar befuddles much of his argument with social science jargon. For example, he attributes the numerous Nazi assemblies in 1931 and 1932 to the fact that the party's "unique standing as an accumulator of traditions of bourgeois apoliticism, obligated the NSDAP to undertake such actions" (p. 205). Although the author discusses "cross affiliation" patterns, the activities of Nazi joiners within the various clubs are by no means clear. Significantly, Koshar has little to say about the numerous Nazi party affiliates that paralleled most of the bourgeois social and professional *Vereine*.

Koshar uses Marburg archival resources, but he virtually ignores the rich biographical data in the Berlin Document Center and other important Nazi document collections used so effectively by Evan Bukey and Geoffrey Giles in their recent local studies. The bibliography lists the Bundesarchiv, the Berlin Document Center, and the Baden state archives, but only five notes are cited from these collections in the book, one of which is a reference to the *Völkischer Beobachter*. A small sample of what Koshar missed includes 1928 propaganda material that reveals specific Nazi plans on how to win the support of a key Marburg *Verein* (National Archives, T-580); the recollections of Gottlieb Braun, a member of Marburg's elite who met Hitler as early as 1923 (NSDAP Hauptarchiv); and correspondence dealing with Professor Wilhelm Pfannenstiel, a candidate of Marburg's *Bürgerliche Einheitsliste* and a future SS officer (National Archives, T-354). The biggest howler of the book involves the author's evaluation of Fritz Schwalm to "concretize the relationship of the NSDAP to local organizational life" (p. 186). Relying on a telephone interview and ignoring Schwalm's papers in the Marburg and Berlin archives, Koshar concludes that "Schwalm lost his enthusiasm for nazism by late 1932" (p. 187), because he rejected "organizational rigidity" and the abandonment of democratic rules in the Nazi party. Even though Schwalm admitted that he became an SS indoctrination officer in 1932, it never occurred to Koshar that the SS combined social and political spheres even more rigidly than did the NSDAP. Indeed, the records reveal that Schwalm was an enthusiastic SS "Racial Evaluation Officer" who was tried and convicted at Nuremberg for his activities in Eastern Europe. With such serious archival deficiencies, many of the book's interpretations are suspect.

JOHNPETER H. GRILL
Mississippi State University

SIMON TAYLOR. *Prelude to Genocide: Nazi Ideology and the Struggle for Power*. London: Duckworth. 1985. Pp. xii, 228. £19.50.

This book by Simon Taylor is both fascinating and frustrating—fascinating in the audacity of its conclusions and frustrating in its standards of scholarship.

Taylor's book examines the evolution of Nazi ideology during the *Kampfzeit*. He argues that its major feature, anti-Semitism, was used as a political instrument to unify the *Mittelstand* behind the Nazi party. Nazi anti-Semitism provided a simplistic explanation for the causes of the social divisions of Weimar Germany. National Socialism, Taylor claims, offered a positive vision of a utopian *Volksgemeinschaft*, a true national community beyond class divisions. Inherent in this positive myth, however, was a negative one. Since the Jews had caused the Weimar crisis, the solution was inevitable—the elimination of the Jews. Indeed, Taylor argues that genocide of the Jews was, in fact, a conscious product of Nazi ideology already in the 1920s.

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Taylor develops his argument in four major sections. Part 1 contains a disjointed survey of German history from the Middle Ages to 1933 and a translation of fifty-four posters and election leaflets from the Rehse Collection in the Bavarian State Archives. The latter are intended to show the development of Nazi ideology and especially its anti-Semitic element. The second section, on the development of the party from 1919 to 1933, examines only three subjects: the Nazi radicals, the party's relationship with big business, and Nazi appeals to different groups within the *Mittelstand*. These three chapters are somewhat superficial and neglect some important works. Conspicuously absent on the relationship between the Nazis and social classes are the works of Michael Kater and Richard Hamilton. The "patterns of political loyalty" of Germany's farmers can hardly be analyzed by using primarily Rudolf Heberle's study of Schleswig-Holstein. Part 3 on the Nazi state examines briefly the betrayal of middle-class interests by the Nazis once in power and the relationship between Nazi political celebrations and Christian practices. The final section treats in only eleven pages the author's major thesis on how Nazi ideology led to genocide.

Overall, the author's impressionistic surveys of certain problems fail to provide adequate basis for his generalizations. One is especially dismayed, however, by a number of factual errors, such as dating the Kapp Putsch in 1919 (p. 21), listing H. S. Chamberlain's middle name as "Steward" (pp. 10, 227), and stating that Theodore Abel "interviewed" the Nazis who wrote essays on joining the party (p. 210). It is curious to see inconsistent translations of the same German passages (for example, compare page 128 with page 29) and "Führer" misspelled repeatedly.

Despite the numerous weaknesses of this book, I remain fascinated by Taylor's conclusions. My own research on the occult origins of Hitler's racial ideology has led me also to ponder the possible truth of Taylor's assertion that "the genocidal impulse of Nazi anti-Semitism was apparent even before the founding of the Third Reich" (p. 217). I would, however, reverse Taylor's line of reasoning that Nazi anti-Semitism was determined "in the first place" by politics and argue instead that the "Jewish question" was primarily a racial one. It is unfortunate that Taylor's conclusions are undermined by the book's weaknesses.

JACKSON SPIELVOGEL
Pennsylvania State University

CLAUDIA KOONZ. *Mothers in the Fatherland: Women, the Family, and Nazi Politics*. New York: St. Martin's. 1987. Pp. xxxv, 556. $25.00.

Enthusiasm permeates Claudia Koonz's book: what her friends call "Claudia's women" have been a consuming interest for a decade. Here, their experience is used to investigate "how decent people [could] adapt to a state that inverted morality, perverted civilized traditions, and imposed distrust on all forms of social life" (p. xxxv). The idea of guilt is never absent, and the clear assumption is that those not actively against Hitler's regime were in favor of it. The leitmotif is Nazi leaders' objective of a "racial revolution," differentiating between "superior" and "inferior" groups: "Aryans" before Jews, Germans before other nationalities, supporters before neutrals, and men before women. This is feminist history that, at least implicitly, accepts David Schoenbaum's designation of Nazi attitudes toward women as "secondary racism." Yet, ultimately, this book is concerned more with the Holocaust than with German women. Its final words explain the eventual testimony of a victim who survived Auschwitz: "because so many Germans had taken up the search for a positive Hitler, she broke her silence" (p. 429).

After a chapter on the Weimar background, including women's alleged "emancipation," the focus is on organized women—those in Nazi, Protestant, Catholic, or feminist organizations, as well as Jewish women and women in the Resistance. There are some interesting nuggets of information, for example about individual women leaders who tried to exert influence through the Nazi women's organizations.

Koonz has visited many archives, consulted a plethora of secondary works, and conducted several interviews, her major coup being one with former Nazi women's leader Gertrud Scholtz-Klink. Her shock at Scholtz-Klink's absence of remorse is disingenuous, since Scholtz-Klink had already exhibited this in both an earlier interview and her book, published in 1978. Further, the claim that here, uniquely, women in the Third Reich are rescued from neglect and obscurity is extravagant: a number of publications about women in Nazi Germany by authors on both sides of the Atlantic—not least in Germany—have appeared fairly recently, as Koonz well knows, since she cites some of them.

This rather disturbing book is riddled with factual inaccuracies and distortions of emphasis. The inclusion of general background, especially on the Weimar Republic, is not intrinsically undesirable—given a tolerant publisher—but the omission of relevant detail, for example on the formation of the Nazi women's organizations and Gregor Strasser's role in it, creates an imbalance. There is throughout a cavalier attitude to points of detail, small or great. Several authors' names have been misspelled, for example, John Hiden is "Hidden," repeatedly; Rolf Eilers is "Eilerts." W. J. Mommsen is credited with

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writing a distinguished book by his brother Hans. Richard Evans's book runs from 1894 to 1933, not 1848 to 1933. Gertrud Bäumer's post was in the Reich Ministry of the Interior, not in a Reich "Education Ministry" (p. 399) or "Ministry of Culture" (p. 147), neither of which existed at the time. Baden was not "a tiny Catholic" state (p. xxvii): its population, larger than Thuringia's and Brunswick's together, was 60 percent Catholic and 40 percent Protestant. In 1932 the Nazi party had eight hundred fifty thousand members (p. 127), not 1.5 million (p. 112). Koonz's words about the movement of the birth rate are downright wrong (pp. 186, 393). Categorical statements are unsupported by evidence in the notes, whereas references to others' work do not sustain the relevant assertions in the text. She seems to appreciate neither the limits to totalitarian rule nor the difference between party and state in the Third Reich: the *Frauenwerk* organization was not a "branch of government" (p. 183). This list of errors is far from exhaustive.

The question is: how many additional errors have I failed to notice? Can one rely on any of the factual or interpretative points in this book? Precisely because Koonz seeks a wider audience than "academe," these errors do matter, since general readers may well take on trust every point she makes and makes so emphatically.

JILL STEPHENSON
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DETLEV J. K. PEUKERT. *Inside Nazi Germany: Conformity, Opposition, and Racism in Everyday Life*. Translated by RICHARD DEVESON. New Haven: Yale University Press. 1987. Pp. 288. $25.00.

West German fascination with "ordinary people" in the Nazi dictatorship has given rise to important research in academic social history as well as new initiatives in nonspecialist history writing. Detlev J. K. Peukert's study of everyday life in Nazi Germany, published in German in 1982 and now available in English, is a product of both currents insofar that the author addresses professional historians, high-school teachers, and individuals involved in adult and political education projects.

Peukert's perceptive work has the twofold goal of synthesizing a growing amount of information on daily life from 1933 to 1945 and analyzing Nazism as "a symptom of the crisis of industrial class society in inter-war Germany" (p. 11). The combination of detail and theory is a useful contrast to much recent West German writing on the subject, which eschews interpretation in favor of recounting "experiences." The first section of the book discusses the accumulating crises of modernization that enabled Nazism to gain power. The second and longest part of the book deals with how National Socialism extended or hindered long-term socioeconomic trends, how National Socialist policies affected major social groups, and how those groups responded to Nazi rule. The third section discusses the Nazi ideology of "*Volksgemeinschaft*, or 'national community,' both as project and as achieved fact" (p. 187).

Peukert aims to deconstruct the widely accepted identification of modernity with progress, a goal that is controversial but not as unprecedented in historical scholarship on National Socialism as the author insists. Peukert skillfully discusses Nazism as a product of the "pathologies and seismic fractures within modernity" (p. 16). He argues that many processes previously considered unique to German development are possible in all capitalist, modernizing societies that are deeply stressed by social, economic, and political conflict. Peukert lays out key features of the German variant of the "worst possible case" (p. 45) of modernity, which included destruction of trade unions and all political opposition, institutionalized inequality, maintenance of the capitalist mode of production combined with regime attacks on big capital, the desperate pursuit of privacy and normality, mass consumption as compensation for loss of meaningful control over public destiny, moral escapism, an ideology of all-embracing national community, mass mobilization and complete ordering of the public sphere, persecution and murder of social and "racial" outsiders, collective lessening of the pain of daily existence through adoration of Hitler, and an expansionist war of annihilation.

The author's discussion of everyday life is intended not to displace political or economic history but to offer a new "angle of vision" (p. 23) of what life was like under the Nazi regime. Of course, there are problems. Peukert offers no satisfactory conceptualization of everyday life, the study of which, the author reminds us, goes well beyond "pointillism and miniaturism" (p. 15). The section on the rise of Nazism is often unconvincing, especially when Peukert relies on Parsonsian theory or refers to "fossilized" (p. 38) artisans and shopkeepers, a description that fits neither the self-image nor the social reality of these groups. Whereas Peukert gives the reader fascinating information on the social environment of manual laborers, the middle classes, and youth gangs, he uses reductionist language when he refers to "working-class and middle-class cultures" (p. 174). Peukert's book has already been outdistanced by current research in some specific problem areas. Nonetheless, *Inside Nazi Germany* is an indispensable text for understanding the social history of Nazi rule.

RUDY KOSHAR
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GEORG LILIENTHAL. *Der "Lebensborn e. V.": Ein Instrument nationalsozialistischer Rassenpolitik.* (Forschungen zur neueren Medizin- und Biologiegeschichte, number 1.) New York: Gustav Fischer or Akademie der Wissenschaften und der Literatur, Mainz. 1985. Pp. vii, 264. DM 48.

Georg Lilienthal's book adds to our knowledge concerning Nazi racial and population policy. It also clarifies the role played therein by a frequently misunderstood SS agency. Yet this study is important beyond its institutional focus, for it examines what "racial experts" trumpeted as the "positive" side of their racial policy—the creation and saving of racially valuable life in contrast to their "negative" agenda aimed at eliminating "inferior races" through sterilization, euthanasia, and extermination programs.

Lebensborn—the "Fountain of Life" foundation established by Reichsführer Heinrich Himmler in 1935—was no SS ruse for concealing "stud farm" activities from the public. For this erroneous interpretation, see the journalistic treatment of Clarissa Henry and Marc Hillel, *Of Pure Blood* (1976). Lilienthal proves that *Lebensborn* maternity homes were an integral component of a eugenics vision held by Himmler and shared by other Nazi ideologues. As such, the homes were justified as a rational response to a perceived population crisis provoked by bourgeois ethical and social strictures. Received wisdom claimed that for material reasons bourgeois convention fostered the postponement of marriage, which ultimately reduced family size. Limiting family growth resulted in population stagnation or decline, unhappy prospects for a regime bent on expansion for living space. Late marriage and fewer children, coupled with the moral and social stigma directed toward illegitimate children, inevitably prompted abortions—the extent of which convinced Himmler that a population and racial crisis was at hand.

Lebensborn therefore sought to subvert bourgeois convention through maternity care to pregnant women, married or not, provided they met established racial criteria. It also extended minimal welfare to large SS families. Exhausting his sources, Lilienthal examines the agency's vicissitudes within and without the SS, including its fight over turf with the party's welfare organ. Organizational and bureaucratic conflict paralleled disappointment at failing to alter bourgeois morality. Determined to obliterate the distinction between public and private morality within the SS, Himmler was usually reluctant, for fear of negative social and religious repercussions, to promote sexual radicalization as a civic duty more widely. Consequently, *Lebensborn* suffered from chronic secrecy and remained primarily an in-house creature, attracting mainly the wives of SS men. Illegitimate births did occur, comprising about half the total registered. Although the *Lebensborn* birthrate conformed to the national average, the illegitimate birth percentage fell well behind the Reich figure. Obviously, SS men had failed to do their "biological duty."

During the war *Lebensborn* extended operations throughout occupied Europe. Lilienthal handles them by locale, delineating foster parent initiatives, the kidnapping of war orphans, and continued involvement with illegitimate births, increasingly justified by German fraternization with the natives. Although these concerns expanded its influence, he concludes that the foundation failed to achieve major results. The negative side of racial policy ultimately overwhelmed it.

The author's treatment of *Lebensborn* during the war is superb. It should become the definitive work on the subject. Moreover, it reinforces the related, though differently focused, works of Michael Kater and Jeremy Noakes. In composite they explain the often banal but always terrible "why" behind Nazi racism.

LARRY V. THOMPSON
U.S. Naval Academy

RICHARD PLANT. *The Pink Triangle: The Nazi War against Homosexuals.* (New Republic Book.) New York: Henry Holt. 1986. Pp. x, 257. $19.95.

In most European countries throughout the nineteenth century and much of the twentieth homosexual acts were defined as criminal. Paragraph 175 of the German criminal code had spelled this out in detail. The pre–World War I youth movement (the *Wandervögel*) had included a significant homosexual component; in the Weimar years homosexuality was widespread and open in Berlin and other cities. Nazi ideologues condemned homosexuality as decadent and immediately after the seizure of power destroyed Magnus Hirschfeld's Institute for Sexual Research, which had dedicated itself to liberalizing punitive laws against homosexuality. At the same time the widely known homosexual activities of Ernst Roehm, the chief of staff of the SA, and the top SA leadership were tolerated by Hitler, who, in an official statement of 1931, stated that sexual matters were of no concern and that "the SA . . . is not an institute for the moral education of genteel young ladies but a formation of seasoned fighters. [The SA man's] private life cannot be an object of scrutiny unless it conflicts with basic principles of National Socialist ideology." In 1931 homosexuality apparently did not conflict with National Socialist ideology. Hitler's decision to destroy (and kill) Roehm and the SA leadership had little to do with homosexuality but centered on

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Roehm's conflict with the Reichswehr and his demand for a "second revolution." Only after the "Night of Long Knives" did Hitler express any shock at Roehm's sexual behavior. Richard Plant correctly notes that the Roehm purge belongs to the category of "political assassinations dressed up as anti-vice action—the only difference being that Roehm had indeed been a practicing homosexual" (p. 109).

In contrast to Hitler, Heinrich Himmler regarded homosexuality as a major danger to Germany and sought to stamp it out; the Roehm affair made this easier. Paragraph 175 was amended to define a variety of activities—kissing, touching, and so on—as criminally indecent. A branch of the Gestapo was devoted to ferreting out homosexuals; many were arrested and sent to concentration camps, where, as Plant shows, they were frequently singled out for special attention. Clearly, homosexuality was a very central concern to Himmler, who wished to destroy all German homosexuals. The prevailing attitude toward homosexuals meant that even in the camps those wearing the pink triangle, the mark of homosexuals, received no support from the antifascist Resistance, which distrusted them. After the war homosexuals (in contrast to other victims of Nazism) were not able to receive any compensation for their persecution, nor are they generally recognized as Holocaust victims.

Together with a history of the treatment of homosexuals in Germany, Plant's work contains a moving personal account (in the introduction and in the conclusion), which maintains that homosexuals deserve recognition as a group persecuted by the Nazis and that their chances of survival were less than that of other prisoners. The work by Frank Rector, *The Nazi Extermination of Homosexuals* (1981), which covers somewhat similar ground, is not cited or included in the bibliography.

GEORGE M. KREN
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STEPHEN PADGETT and TONY BURKETT. *Political Parties and Elections in West Germany: The Search for a New Stability*. Rev. ed. New York: St. Martin's or C. Hurst, London. 1986. Pp. xi, 308. Cloth $32.50, paper $14.95.

Stephen Padgett and Tony Burkett have made a useful and sometimes provocative contribution to the literature on German political life. Following a historical survey with a focus on the Weimar Republic, the study provides an analysis of the structure and functioning of political parties in the Federal Republic and concludes with a sociopolitical profile of the electorate. Addenda on such subjects as party financing are helpful. Although published before the Bundestag (parliamentary) election of 1987, the careful organization of a wealth of detail makes the book essential reading for those concerned with the future of German democracy.

Two conceptual problems do, nevertheless, confront the reader. The first derives from repeated references to both the Social Democratic party of Germany (SPD) and the Christian Democratic Union (CDU) as *Volksparteien*, that is, parties appealing to all strata of society and therefore not identified with narrow class or sectarian interests. Contrary to this description, the authors introduce their subject with the assertion that the era of "hyperstability" is coming to an end because voters have begun to question the governmental efficiency and responsiveness of the two large centrist parties. Protest movements, notably the Greens, are the result. The balance of the discussion is, however, at variance with the thesis. If it is true that popular mistrust of the party structure weakens consensus, why do the SPD and the CDU continue to gravitate as close as they can to the middle of the political spectrum? We read that the pragmatic leadership style of the Social Democratic Chancellor Helmut Schmidt (1974–82) was politically more popular than the ideological image of his predecessor, Willy Brandt, who was the SPD's chairman until 1987. Similarly, Helmut Kohl's moderate policies have made him a more successful leader of Christian Democracy than his Bavarian rival, Franz-Josef Strauss. The conclusion is that West German voting behavior reflects a consensus in support of a managerial approach to government. The authors suggest that old ideological cleavages may reassert themselves, but the data do not lend credibility to this forecast.

The second problem is that the authors make little effort to link the SPD and CDU to a general theory of political development. For this purpose the growth of participatory democracy in postwar Germany could offer an interesting case study. The "Americanization" of political campaigns and the bureaucratization of party structures are themes of the book, but these notions are not tied to the theoretical literature on oligarchical tendencies within political movements. Even the Greens are beginning to organize a hierarchical pattern of party leadership in contrast to the loose coalition of protest groups represented in the federal election of 1983. For the historian concerned with the role of political parties in bringing about the rise of the modern state, a concluding discussion of political mobilization following defeat and occupation would have been useful. In sum, the authors have given the specialist a comprehensive interpretation of present-day West German electoral politics, but the

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generalist in European history would have benefited from a somewhat more theoretical approach.

JAMES H. WOLFE
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R. BURR LITCHFIELD. *Emergence of a Bureaucracy: The Florentine Patricians, 1530–1790.* Princeton: Princeton University Press. 1986. Pp. xiii, 407. $47.50.

Anyone who has worked with the administrative records of Florence from the period of the grand duchy will appreciate the prodigious labors of R. Burr Litchfield. This book is a *histoire totale* of the ruling class from the Renaissance republic through the eighteenth century. Providing more than merely an administrative history, Litchfield lays out with statistical rigor and wide reading in the sources the demographic, economic, and cultural basis of this political history. He shows that state building during the period of the grand dukes has been obscured by those who claim a "too-early point of arrival" for the Renaissance or "too much of a break" for the later period (p. 4). Although the mid-sixteenth-century reforms of Cosimo I did not flood the government with new men (as Antonio Anzilotti long ago claimed), the grand dukes made advances in bureaucratic organization that went beyond the precocious roots grounded in the Renaissance republic. Power shifted from the rotating offices staffed by noble *magistrati* to permanent ones held by functionary *ministri*. The change, however, did not spell the demise of the old Florentine magnates. Instead, with the economic crisis of the seventeenth century, patricians began to dominate the ministerial positions, as the political economy of the Medici shifted from protecting the interests of Tuscany as a whole to favoring the special interests of the Florentine nobility. The revolution in the officeholding class had to await the post-Medicean government of the Lorrain regency—the reforms of Francesco Stefano in the 1740s followed by the antiaristocratic policies of Pietro Leopoldo in the 1770s and 1780s. Only then did Tuscany make the transition from the "patrimonialism" of officeholders with a stake in the ruler's household to the "rational-legal state."

If this book merely chronicled the development of the early modern state of the grand dukes, it would merit applause. But the work poses big questions that go beyond specialist interest in Florentine history. Throughout, Litchfield places the Tuscan developments in wider historiographical contexts, comparing Tuscany with Milan, Naples, Venice, the House of Savoy, Prussia, and France. Litchfield makes use of Weberian distinctions but at the same time exposes their shortcomings. Under the broad rubric of "patrimonialism," which could describe the late medieval city-state as well as Medicean "absolutism," Litchfield shows critical changes in the structure of the Tuscan state. Further, he effectively challenges notions that national states emerged from feudal absolutism and that the development of the Italian state from city-states to Risorgimento was a history of false starts. According to Litchfield, historians have overstated the refeudalization of Tuscany of the seventeenth century. The grand dukes pursued policies aimed at strengthening the mercantile interests of Florence, and the city never relinquished its commercial dominance over the countryside. Nor did the Medicean experience of the grand dukes stall state building; rather, the modern state continued to evolve steadily throughout the early modern period. In conclusion, if Litchfield's book receives the attention it deserves, Eric Cochrane's epithet, "Florence in the forgotten centuries," should lose its appropriateness.

SAMUEL COHN, JR.
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JUDITH C. BROWN. *Immodest Acts: The Life of a Lesbian Nun in Renaissance Italy.* (Studies in the History of Sexuality.) New York: Oxford University Press. 1986. Pp. viii, 214. $6.95.

Dedicated to the religious life at birth, Benedetta Carlini left her mountain village at the age of nine to enter a convent in Pescia, the provincial center. In the cloister she suffered paralyzing pain and soon distinguished herself from the other nuns by following a regimen of extreme fasts and experiencing supernatural apparitions. At first the Madonna gave her signs of special favor, but, as she grew older, male figures came to dominate the visions—a beautiful youth, young men who beat her with chains, sticks, and swords, a handsome guardian angel named Splenditello, and Jesus himself. Such mysterious experiences soon brought a certain local notoriety. After showing signs of the stigmata, she was elected abbess, and her vocation as mystic and celebrity blossomed. In one episode Jesus took out her heart and replaced it three days later with his own; on a later occasion in an elaborately staged public ceremony where only she saw the bridegroom, Abbess Carlini married the adult Christ (rather than the infant Jesus as had her model, Saint Catherine of Siena); and in her climactic scene she "died" and was resurrected.

The unusual wedding prompted the first of two official investigations of her that took place between 1619 and 1623. The ecclesiastics eventually discovered evidence that she had faked the stigmata by pricking herself with a needle, secretly eaten salami and mortadella during her fasts, painted on her miraculous wedding ring with saffron, simulated her own death, and forced or seduced another nun

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into mutual masturbation and oral sex. Although no record survives of a judicial sentence, Judith C. Brown discovered that after thirty-five years imprisoned in the convent Benedetta died still revered by the populace.

Benedetta Carlini's story is a gripping one of collective delusion, jealousy, ambition, and lust in which a clever woman acted out the part of a saint in a culture that anticipated spiritual influence in human events and expected divine truths to be revealed through visual signs. Brown analyzes with clarity and sensitivity the theological and monastic context for Benedetta's visions and respects the desire of so many persons to believe in them. In this second volume of Oxford's Studies in the History of Sexuality series, however, the significance of sexual behavior is more difficult to determine. The introduction focuses exclusively on lesbianism in the history of Christian thought and law disclosing the difficulty male ecclesiastics had in classifying or even imagining sexual relations between women. But one reads very little more about Benedetta's sexual experiences until the end of the last chapter, and Brown admits that the long punishment was more likely due to the nun's violation of her monastic vows, her spurious saintliness, and her dangerous popularity with the laity than her sexual sins. Her lover, apparently, suffered no severe penalties. Benedetta's mystic experiences of handsome men who tortured her and her assumption while making love of a male persona complete with a disguised voice and a masculine angelic name testify most of all to a confused and ambiguous identity. The liaison shocked the investigating team, but evidence of the nuns' sexual behavior was seemingly discovered by accident and merely served to confirm a verdict of diabolic influence. The remarkable thing about this case of a woman from a society highly aware of matters corporeal is how little sexuality, especially female sexuality, in contrast to spirituality really mattered to anyone other than the lovers themselves.

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GIUSEPPE DE LUTIIS. *Storia dei servizi segreti in Italia.* (Politica e società, number 40.) Reprint. Rome: Riuniti. 1985. Pp. 313. L. 16,500.

This is an account of Italy's secret services from the Fascist era (including a brief survey of the 1863–1919 period) to the present. Giuseppe De Lutiis, a journalist and teacher, bases his account largely on secondary works, newspaper and periodical sources, and judicial and parliamentary proceedings.

Any reader expecting more than has heretofore been available on *Ovra* (*Opera vigilanza repressione antifascismo*), Mussolini's secret political police, will be disappointed. De Lutiis is a little more detailed in discussing the military service's *Sim* (*Servizio informazioni militari*), established in 1925 and dissolved in 1945. He gives considerable attention to *Sim*'s responsibility for the assassination of the Roselli brothers in France in June 1937.

The author contends that the reestablishment of secret services in Italy after World War II was hindered by the United States, because the latter was unsure of the political orientation of the new republican Italy. It was only after the elections of April 1948 and Italy's subsequent adhesion to the North Atlantic Treaty Organization (NATO) that Italy's first postwar secret service was born, *Sifar* (*Servizio informazioni forze armate*). Former members of *Ovra*, alleges De Lutiis, passed into the secret service of democratic Italy. In 1955 General Giovanni De Lorenzo, not a former *Ovra* member but of questionable personal integrity, became head of *Sifar*, a position he held until 1962. According to De Lutiis, *Sifar*'s collaboration with the Central Intelligence Agency was very close, embracing even the bugging of the pontifical library.

In describing the years 1969 to 1975, De Lutiis devotes his attention to secret organizations that paralleled official organizations and that were designed to prevent a Communist takeover of Italy, even if it meant attacking the democratic republic. Many individuals, civilian and military, were involved in these organizations, which were also in collusion with NATO. In 1975 this subversion of the Right gave way to subversion of the Left, with some members of the secret Right organizations passing into the ranks of the Red Brigades. The book ends with a discussion of the reform of the secret service enacted by Parliament in 1977. As the author points out, the reform did not come soon enough to help Aldo Moro, who was kidnapped and murdered by terrorists in 1978.

This book does not make for smooth reading. There is a choppy quality to it, with organizations, individuals, and specific crises described somewhat haphazardly. Overall, the book is a useful introduction to Italy's secret services, but it is neither an exhaustive nor a fully objective treatment of the subject.

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GUILLAUME DE BERTIER DE SAUVIGNY. *Metternich.* Paris: Fayard. 1986. Pp. 535. 150 fr.

I expected Guillaume de Bertier de Sauvigny's biography of Prince Klemens von Metternich to pro-

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vide an authoritative survey of his life and times for the general historian but not to offer much new to Metternich scholars. The first part of this expectation proved correct; the second was wrong. Bertier's general picture of Metternich's personality and policies will naturally be familiar to those who know his earlier studies. Nonetheless, even specialists will find new information and insights in this masterly biography.

Of Bertier's sovereign command of the sources, nothing need be said; the work reflects it on almost every page, although it has few footnotes and little bibliography. It also exemplifies the standard virtues of Bertier's style—serene, urbane, and witty. The perennial problem facing biographers, how to integrate the personal and public lives of their protagonists, is handled admirably for three-fourths of the book up to about 1825. In this section Bertier skillfully weaves together Metternich's public acts and private life, including his love affairs, demonstrating their pertinence to many important and controverted developments—for example, Austro-French relations before the war of 1809, Metternich's futile efforts to win concessions from France in 1809–12, his policy of armed mediation in 1813, and his role at the Congress of Vienna. The biography becomes a solid account of the high politics of the era, with the major political developments of the time described with lucidity and compression.

After 1825, when Metternich lost command of affairs as the coachman of Europe, one gets the feeling that Bertier also loses interest to some degree in the wider picture. The many quotations from the correspondence of Metternich and others, almost always interesting and apropos earlier, tend in this later section sometimes to substitute for narrative and analysis rather than to enhance it. Some important political subjects (for example, Metternich's sabotaging of the Petersburg Conference in 1825, his fight against Mazzinian subversion and revolution in Italy and Switzerland, his role in various German issues of the 1830s, his influence on Francis Joseph and the restoration of absolutism in Austria after Metternich's fall) are slighted or overlooked in favor of fairly routine details on Metternich's travels and personal affairs. Yet here too there are important insights into major developments in this era of the decline of Metternich and his system. In short, this is throughout an interpretation of Metternich's world, not merely an account of his life.

How convincing an interpretation is it? In mastery of the facts and portrayal of the man himself, entirely so. Metternich appears here, as in Bertier's other works, as unflappable, urbane, immensely vain, less pleasure-seeking and selfish than accustomed automatically to expect life's goods to come his way, not frivolous or corrupt but sometimes superficial, concealing (even to himself) his actual uncertainty, wavering, and repeated changes in position behind a pose of infallibility in judgment and unwavering adherence to principle, and, above all, endowed with the ability to wait on events, beat skillful retreats, cover his tracks, and make the best of a bad job. Bertier's portraits of many of Metternich's contemporaries strike one as similarly acute and profound—those of Alexander I, Francis I, Talleyrand, and the women in Metternich's life, for example.

As for the overall interpretation of the Metternich era, especially its high politics, almost all historians will find Bertier's views equally sensible and convincing. Metternich is certainly not his hero, but he recognizes his skill, good sense, and rationality and gives him full marks for his success in restoring Austria to greatness and in creating a Germany tolerable for Austria and Europe. In other words, his is a sane, centrist view avoiding both the liberal excesses of the nineteenth- and earlier twentieth-centuries and the neoconservative ones of the post–World War II era. For various reasons too complicated to go into here, some of the broad lessons and ideas I derive from Metternich's career diverge from those of Bertier. But it is a sign of the indispensable character of Bertier's work that even differing interpretations must lean on his scholarship.

PAUL W. SCHROEDER
University of Illinois

KARL HEINK STREITER. *Die nationalen Beziehungen im Grossherzogtum Posen (1815–1848)*. (Geist und Werk der Zeiten, number 71.) New York: Peter Lang. 1986. Pp. viii, 181.

"Through German culture alone can the Polish population be raised up out of the depths into which it has been dragged, unfortunately in large measure through its own fault, by misery of all descriptions" (p. 96). So wrote in 1841 the mildly liberal governor of the province of Posen (Poznań), the heartland of Prussian Poland. His remark epitomizes the supercilious attitude toward the Poles ingrained in Prussian officialdom since the days of Frederick the Great. Karl Heink Streiter argues that this mentality, coupled with an excessive suspicion of the Poznanian Poles' revolutionary potential, prevented the Prussian government from successfully integrating them into the state.

Streiter shows how, despite the Prussian government's rebaptism of the province in 1815 as the "Grand Duchy of Posen" (complete with a Polish "viceroy"), official policy quickly moved toward the Germanization of public institutions and higher education. This weakened the ability and desire of

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the Polish nobility and noble-born intelligentsia to enter public service in their homeland. But, unfortunately for the government in Berlin, the Polish nobility displayed a talent for strengthening itself outside the walls of institutional power. At the same time, it successfully recruited in increasingly large numbers the children of the common people into the nationalist intelligentsia. By 1848 governmental hopes that Polish national consciousness could be confined to the manor houses of anachronistic squires were doomed.

Streiter's treatment of these themes, as also of church-state relations, the province's parliamentary life in the pre-March decades, and the Poznanian revolution of 1848, offers an accurate and up-to-date synthesis of the literature, underpinned by solid archival work and complemented by good statistical tables. For the most part, the book refines and deepens preexisting analyses. Streiter breaks new ground in documenting the strength of anti-Polish German national consciousness, though not in all circles, on the eve of the revolution of 1848. But he upholds the established view that, in their political relations with their Polish neighbors, most of the Poznanian Germans and Jews rejected official discrimination against the Poles. Before and during 1848, they preferred to trade support of Polish national interests (short of independence) for Polish backing in the movement for constitutional rule in Prussia.

But the relations of the nationalities extended beyond political compromises, and on Streiter's own evidence (including the violence of 1848) the Poles, Germans, and Jews did not live together in sweet harmony. Research on Prussian Poland has, understandably, centered on the emergence of official Germanization programs and the Poles' resistance to them. But, surely, one of the keys to understanding this antagonistic development lies in a closer study of the nationalities' daily interactions and their separate, often deeply conflicting mentalities.

WILLIAM W. HAGEN
University of California, Davis

STEVEN BELA VARDY. *Baron Joseph Eötvös: A Literary Biography*. (Atlantic Studies on Society in Change, number 52; East European Monographs, number 114; Social Science Monographs.) Boulder, Colo.: East European Monographs, or Hungarian Cultural Foundation, Stone Mountain, Ga.; distributed by Columbia University Press, New York. 1987. Pp. viii, 189. $25.00.

In the 1860s, the Hungarian statesman-philosopher, Baron Joseph Eötvös, extolled individual freedom and social justice over and against both excessive equality, which he thought could lead to popular despotism, and excessive state power, the mark of tyranny. Ideally, Eötvös hoped, the impact of the state on the individual would resemble atmospheric pressure, commonly unnoticed unless it increases or becomes uneven.

Eloquence, wisdom, and a desire for balance were the trademarks of the mature Eötvös, yet his life was anything but measured. A romantic in his youth who brought the Hungarian reading public to tears with his sentimental novel *The Carthusian*, Eötvös turned to social criticism in the 1840s through fiction as well as an exposure of his country's backwardness. A promoter of the most advanced liberal ideas, he nevertheless recoiled from revolutionary violence in 1848. A patriot who perceived the protective role of Austria in Hungary as necessary, a devout Catholic who championed the emancipation of the Jews, an aristocrat who felt sympathy for the poor and understanding for non-Hungarians in the country, Eötvös, the liberal humanist, is an attractive figure in Hungarian history.

This attractiveness certainly plays a role in the appeal Eötvös has had for several biographers. Steven Bela Vardy's portrayal of Eötvös is well written and lively. The best parts deal with Eötvös the writer, appropriately so considering the book's subtitle. The author enlightens the reader about lesser-known aspects of Eötvös's creative work such as his poetry, and splendid lithographs add color and atmosphere to the narrative.

Some problems arise with Vardy's evaluation of Eötvös as a statesman. The most critical point in Eötvös's life was his desertion of the national cause in 1848. This action can be condoned, condemned, or argued about, but its discussion should not be evaded. Vardy sees the problem but claims that he accepts Eötvös as he was without needing to pass judgment on him. Much as a biographer likes his subject, and Vardy clearly does, controversial issues have to be confronted rather than passed over in silence. Although it is possible to be simultaneously a revolutionary and a gentle dreamer—Peter Kropotkin comes to mind—this was not the case with Eötvös, as Vardy claims. Eötvös was a left liberal thinker who abhorred violence and therefore revolution.

Apart from these objections, this book is worthy of the man whom Vardy rightly calls "the most truly 'European' Magyar in nineteenth-century Hungary" (p. 152).

GABOR VERMES
Rutgers University, Newark

ENDRE KISS. *Der Tod der k.u.k. Weltordnung in Wien: Ideengeschichte Österreichs um die Jahrhundertwende.*

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(Forschungen zur Geschichte des Donauraumes, number 8.) Vienna: Böhlau. 1986. Pp. 296. S 340.

This deeply reflective book presents a rethinking of Austria's intellectual renewal between 1890 and 1918. Through essays of three to twelve pages on about twenty-five thinkers, the Hungarian historian Endre Kiss offers a new conceptualization of Austrian thought. Omitting all reference to structuralism, Marxism, or social history, he redefines categories that predecessors such as Hermann Borch and Albert Fuchs developed. Since Kiss wrote this book (in Hungarian) during 1975–78 and revised it for this German translation, it reflects almost no research from the last ten years. Withal, Kiss introduces more than a few useful concepts and suggests novel ways in which familiar writers mirrored Austrian conditions.

Kiss differentiates three modes of Austrian thought. The seventeenth and eighteenth centuries implanted a universalist (or "harmonistic") outlook that sought not just to juxtapose but to reconcile opposites. Modernism arrived late in Austria, emerging during the 1890s after the first wave of European rethinking (in Friedrich Nietzsche, Stéfane Mallarmé, and Henrik Ibsen) had crested. More than half the book unfolds a highly original analysis of Austria's second mode of thought, which Kiss calls the second Enlightenment. Kiss links Viennese positivism not with Auguste Comte or Herbert Spencer but rather with the more humane currents of the French Enlightenment. Viennese love of harmony made the second Enlightenment of the 1890s more concrete and less scientistic than elsewhere. Protagonists of this outlook included Joseph Popper-Lynkeus, Friedrich Jodl, and Max Nordau. Viennese immersion in the concrete gave rise to impressionism, a literary stance that could invest any moment with universal significance. Comparisons between Peter Altenberg, Hermann Bahr, and Otto Weininger constitute some of the richest pages in the book. A dazzling display of dialectics accompanies Kiss's discussion of the young György Lukács. Kiss argues persuasively that if Lukács had died in 1918 he would constitute for us a discovery as startling as that of Ludwig Wittgenstein, Robert Musil, or Hermann Broch (p. 183).

Kiss's ultimate contribution concerns Austria's third mode. Rather than interpret Ernst Mach, Karl Kraus, Franz Kafka, Wittgenstein, or Broch as deriving from positivism or impressionism, Kiss places them in a different context. These explorers of the modern strove to discern new constituents at the core of reliability. They introduced fundamental variables ("elements") with which they articulated new systems for conceptualizing concrete experience. An example is the way in which Kafka postulated laws not just in one realm such as jurisprudence, religion, or metaphysics but in all realms at once. In Kafka's world, there is always someone who rebels against these pervasive laws, which become known only through struggle against them. Kafka states Austria's age-old preoccupation with world order in terms of a world order against whose silence we moderns still struggle.

Although the book abounds in introductory passages, it suffers from a lack of concluding ones. The text ends abruptly with Broch, just as many sections end in midargument. The proliferation of mini-essays of ten to twenty lines in the footnotes adds to a certain fragmentariness. Happily, this is compensated by the frequency of references to little-known Hungarian commentators, whose writings Kiss could assemble into an equally useful book on Austrian culture seen from Hungary. It is to be hoped that the present book will spark interest not only in its reconceptualizations but in the study of parallels and divergences between Vienna and Budapest.

WILLIAM M. JOHNSTON
University of Massachusetts

NEAL PEASE. *Poland, the United States and the Stabilization of Europe, 1919–1933.* New York: Oxford University Press. 1986. Pp. vii, 238. $24.95.

In the words of the author, this book is "a detailed account of something that did not happen" (p. vi), the "something" in this case referring to the creation of a solid economic and diplomatic partnership between the United States and Poland during the interwar period. In essence, it is a book about failure: the failure of a geopolitically vulnerable Poland to attract significant American capital investment and diplomatic interest and, perhaps more important, the failure of American "dollar diplomacy" to enhance Polish security and stabilize an economically and politically fragile Europe. In his thoughtful analysis of Polish-American relations during the crucial years of the 1920s and early 1930s, Neal Pease has provided a welcome contribution to the diplomatic history of the period, one that sheds considerable light on the nature and limitations of the American presence in interwar Europe.

Pease places his study squarely within the context of recent scholarship on U.S. policy in Europe during the administrations of Calvin Coolidge and Herbert Hoover, which views that policy's primary goal not as seclusion and isolation but as "stabilization" through the expansion of American financial influence coupled with disinterested mediation of European political and territorial disputes. Announced by U.S. sponsorship of and commitment to the Dawes Plan of 1924, the American decision to reclaim a position of importance in Europe quickly

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attracted the attention of the hard-pressed Poles, who naturally considered their own economic vitality and national security key elements to general European stability.

Poland's subsequent search for the "golden fleece," its often frenetic efforts to entice American dollars, ended largely in frustration. Although Poland was able to secure a relatively significant stabilization loan in 1927, engineered behind the scenes by Benjamin Strong of the Federal Reserve Bank, Polish expectations of much more substantial American investments never materialized. With an economy plagued at various times by runaway inflation, a debilitated currency, chronic budget deficits, and sizable war debts, Polish governments found it difficult, if not impossible, to lure American capital to the country. Domestic political instability, at least until the advent of Józef Piłsudski's *Sanacja* regime in 1926, did little to improve the country's credit standing in the eyes of American investors. External factors also played a role: the stock market boom of 1928 followed by its crash a year later generally dried up the flow of American dollars to Europe, while the great global depression forced Poland to join a long list of states that defaulted on their international payments. Most important, however, Poland could never overcome the reluctance of American financial circles to underwrite a state surrounded by two powerful and hostile neighbors, Germany and the Soviet Union. Anticipating that American diplomatic support would follow American capital into Poland in order to protect Wall Street's investment, the Poles in the end received neither. Quite simply, Poland could not shed the overriding perception in New York and Washington—borne out in September 1939—that it was a poor economic and political risk.

Pease's study has much to recommend it: the author's meticulous research in archives on both sides of the Atlantic, his revealing portraits of the principal Polish and American actors, his careful emphasis on the larger European context of Polish-American relations. On the other hand, the book could benefit from a more thorough discussion of the new literature on American stabilization policy in Europe, especially given the acknowledged importance of "stabilization theory" in shaping Pease's own project. One also wishes that Pease would have taken his study up to 1939, even though the policy of stabilization had long since played itself out. By the same logic, the sharper focus accorded by the stress on stabilization should have excluded treatment of the period 1919–24. These problems, however, detract only marginally from the overall value of the book, a must read for students of American diplomacy in interwar Europe, as well as for those concerned with the external relations of independent Poland.

ROBERT BLOBAUM
West Virginia University

CHARLES GATI. *Hungary and the Soviet Bloc*. Durham: Duke University Press. 1986. Pp. 244. Cloth $40.00, paper $14.95.

Although many of the essays in this volume have been published before, together they provide a finely documented, if not comprehensive, history of Hungary since 1944. The first five chapters concern the "takeover" period—1944–48—and are especially valuable. They reflect the mountainous evidence that Stalin at the end of the war did not expect, or want, Hungary to be converted into the Communist-ruled satellite it became after 1950. Maybe in ten or fifteen years, but not in 1944–45! Charles Gati explains Stalin's stance conventionally in terms of an allegedly "traditional" two-tactic character of Soviet foreign policy, which Stalin "surely" or "apparently" manipulated. But that Hungary's fate, and that of much of the rest of the Eastern Europe, was in doubt until 1947 can no longer, after this book, be contested.

In meaty chapters on the social background of the end-of-the-war upheaval, on the political coalitions of the period, on the Jewish question, and on the great political "turn" of 1947–48 Gati shows how inconvenient the Soviet policy was to the Hungarian Communists. He equivocates a bit here, because on the one hand he is addressing a naive American audience that has to be told that Hungary was in general eager for revolutionary change in 1945, yet on the other hand he is addressing a too sophisticated Hungarian world that, for example, is deeply opinionated about (though not always against) Jews. Still, his main points come across well: everyone in postwar Hungary was scared. No one knew how the game would end. Yet real hope and enthusiasm were in the air.

Two further chapters concern the Soviet betrayal of Imre Nagy in 1956 and János Kádár's cautious manipulation of Moscow politics since then. Both are replete with local insights and for this reason well worth reading. Much the same may be said about the final three chapters, which deal broadly with the possibilities of change in Soviet foreign policy, with the loosening of Soviet–East European relations, and with the United States' policy of encouraging diversity in Eastern Europe. Over and again Gati draws on his conversations in Budapest to construe that the Russians have always been, and will remain, Russians but that there is nonetheless a lot of room for cautious innovation in Eastern Europe and a lot of reason therefore for Western

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interest in "coexistence." All told, this is a stimulating and useful little book that, because it is disjunctive, avoids many of the usual failings of Sovietology punditry.

WILLIAM O. MCCAGG, JR.
Michigan State University

N. S. BORISOV. *Russkaia tserkov' v politicheskoi bor'be XIV–XV vekov* [The Russian Church and the Political Struggles of the Fourteenth and Fifteenth Centuries]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1986. Pp. 205. 1 r.

This book is unabashedly propagandistic: N. S. Borisov intends to contribute to "scientific-aetheistic" propaganda on the topic of church-state relations in the fourteenth and fifteenth centuries. He aims to discredit the "pseudoscientific," "anti-Soviet," "clerical falsification" of "bourgeois" scholarship on this issue, namely a tendency to overemphasize the "intrinsic religiosity" of the Russian people and the role of the church in the achievement of Muscovite centralization (he cites scholars of a previous generation, including N. Zernov, G. Fedotov, D. Obolensky, N. Andreev, and Suzanne Massie). Borisov's tendentiousness is unfortunate. The history of the church in Russia has been virtually neglected by Soviet scholars and deserves more constructive treatment than is found in this book.

Borisov sets out to define the "political line" that metropolitans adopted toward the state in the fourteenth and fifteenth centuries. He finds it not to have been slavishly loyal to Moscow. He reviews well-known episodes of church-state relations from this perspective, stressing fourteenth-century metropolitans' involvement with Constantinople and other Rus' princes. He argues that monasteries more than the metropolitan's seat provided ideological support for Moscow's rise and asserts that the state politically emasculated both metropolitan and monasteries by the end of the fifteenth century.

In criticizing outdated scholarship, Borisov has raised a straw man: few Western scholars would disagree that the views he cites are extreme and romantic. But Borisov ignores recent Western monographs and articles (for example, by J. L. I. Fennell, J. Meyendorff, G. Alef, D. Goldfrank, D. G. Ostrowski, J. Kollmann). Borisov's recognition that the church had aims other than to support Muscovite centralization is well taken but hardly novel. And, having criticized "idealistic" historiography, he still argues more in terms of personalities and historical inevitability than of material forces. The book, a chronological narrative of power struggles between specific metropolitans and grand princes, is overly personalized given how little is known of those individuals. The book also lacks depth: Borisov fails to explain how grand princes engineered support to depose a metropolitan or to appoint a favorite or how metropolitans mobilized opposition to block such moves. The book is also unbalanced: it does not admit the church's significance as a source of historical continuity, moral authority, and political integration during Moscow's unification of northeast Rus'.

Borisov perpetuates ideas that are being reconsidered, such as the assumption that church-state relations were antagonistic, that the acquisitor-nonacquisitor debate was politically significant, and that Ivan III planned to secularize church lands in 1503. On the first point Borisov might have profitably emulated V. B. Kobrin's questioning of the conflict model for early modern Muscovite politics. One could argue that church-state relations should be characterized as symbiotic and cooperative.

Borisov does offer some new work. He has correlated the chronology of political struggles and major church and secular building projects, and he has analyzed the political symbolism of architectural styles and of the saints to whom new buildings were dedicated or on whose namedays important ceremonies took place. He proposes that such naming, dating, and choice of styles promoted religious or secular cults in support of Moscow's centralizing plans. Particularly in the book's earlier chapters, this approach provides some interesting, although speculative, insights. Unfortunately, Borisov's polemical tone overwhelms his scholarly efforts.

NANCY SHIELDS KOLLMANN
Stanford University

JAMES F. BRENNAN. *Enlightened Despotism in Russia: The Reign of Elisabeth, 1741–1762*. (American University Studies, ninth series, History, number 14.) New York: Peter Lang. 1987. Pp. 295. $44.50.

The only constructive thing I can say about this book is that it offers an object lesson in what scholars, teachers, and students should avoid. The text shows no sign of editing. Virtually every page displays glaring typographical, spelling, or grammatical errors (at least ten on pages 196–97, for instance). The content is poorly informed, confused in presentation, and mediocre in style. There is no index. The sloppy bibliography overlooks numerous relevant works in Russian and English. Many Russian titles and terms are mangled or mistranslated. Although James F. Brennan aspires to rehabilitate Elizabeth, we gain no insight into her personality, motivations, or reign. In short this work is a travesty of historical writing I regret having rec-

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ommended (sight unseen) for purchase by my university library.

J. T. ALEXANDER
University of Kansas

HUGH D. HUDSON, JR. *The Rise of the Demidov Family and the Russian Iron Industry in the Eighteenth Century.* (Russian Biography Series, number 11.) Newtonville, Mass.: Oriental Research Partners. 1986. Pp. ix, 166. $30.00.

In the eighteenth century the Demidov family of Russia opened more than fifty manufactories for the production of iron and copper. They supplied the Russian state with approximately half of its iron and made Russia the largest producer and exporter of iron in the world.

In this slender volume Hugh D. Hudson, Jr., traces the history of the Demidov family from its origins as artisans and religious dissenters in the late seventeenth century through its creation and domination of the Ural iron industry in the eighteenth century to its total absorption into the Russian nobility and its alienation from metallurgy and enterprise in the early nineteenth century. The first two chapters, dealing with the origins and rise of the Demidov family, are the weakest part of the book in both style and content. The remainder is far more confidently written and informative as the author makes effective use of his extensive archival research.

Although he deals in passing with a number of interesting and important issues, the author concentrates on two in particular: the Demidovs' relations with the imperial government and their role in Russian society. On the first he argues persuasively that the state provided the Demidovs' manufactories with markets, concessions, and an obligated labor force that enabled the iron industry to surmount the obstacles that prevented the development of other industries. It was a defense industry deliberately fostered by the state and not a natural outgrowth of the Russian economy as a number of Soviet historians have argued. On the second issue the author argues with considerable skill and originality that eighteenth- and nineteenth-century Russian society had no place for a family of wealthy industrialists because there was no class or subculture to sustain its values. There the author is at his best in tracing the gradual, sometimes reluctant, but ineluctable merger of the Demidov family into the Russian nobility over several generations and its concomitant withdrawal from industrial enterprise.

An introduction by David Griffiths enhances the value of this brief but engaging book. In twenty-six pages Griffiths clearly and expertly places Hudson's work in the context of Soviet historiographical controversies over the role of manufactories in the rise of capitalism in Russia. Regrettably, the book does not contain an index.

ROBERT E. JONES
University of Massachusetts

ROBERT H. MCNEAL. *Tsar and Cossack, 1855–1914.* New York: St. Martin's. 1987. Pp. 262. $27.50.

During the February revolution of 1917 an unexpected incident took place. A detachment of Cossacks, summoned to restore order, declined to repeat their performance of 1905, showing none of their customary zeal in dispersing the crowds. On the contrary, according to eyewitnesses, a Cossack officer winked at a group of demonstrators in token of sympathy and support. By refusing to fire, the Cossacks tipped the balance in favor of the revolutionaries. In so doing, they sealed the doom of the autocracy, of which they themselves had been a principal buttress.

How could such a thing have come to pass? Why did the Cossacks, the very symbol of Tsarist repression, cast in their lot with the insurgents? Robert H. McNeal's new book seeks to provide an explanation. The Cossacks, according to McNeal, were the product of a dual heritage—or of two contradictory "myths," as he puts it. The first was the myth of "tsar and Cossack," whereby the monarch granted the Cossacks land and privileges in return for unswerving loyalty. This myth predominated throughout the nineteenth century, when the Cossacks stood as pillars of imperial authority, preserving internal order while also securing the empire's borders against invasion. Previously, however, a very different myth had characterized Cossack behavior, the myth of grass-roots democracy and social justice. Between 1600 and 1800, the Cossacks had been the champions of the downtrodden elements of Russian society, providing military leadership for popular rebellions (those of Stenka Razin and Yemelyan Ivanovich Pugachov being the most famous) as well as a spirit of freedom and model of independence that posed a challenge to the tsarist regime.

It was to this earlier heritage, argues McNeal, that the Cossacks reverted in 1917, siding with the people against the government. This should not have been surprising, for, despite their conspicuous role in suppressing the revolution of 1905, the Cossacks had been nurturing grievances that undermined their devotion to the tsar. Not least among these was the rising cost of military equipment, which, given the inadequacy of government subsidies, placed an intolerable burden on Cossack finances. To this must be added a declining Cossack economy, an influx of non-Cossacks into Cossack territories, a widening gulf between conservative and liberal Cos-

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sacks, and a growing disinclination on the part of many Cossacks to engage in police operations. By the beginning of the twentieth century, says McNeal, the Cossack system was already an anachronism. By the outbreak of World War I, it stood on the brink of collapse.

To readers familiar with the pertinent literature (nearly all of it in Russian), much of this analysis will seem familiar. McNeal makes no pretense of having unearthed fresh historical information. Basing his work almost exclusively on printed sources, he is content to synthesize and explain. In doing so, however, he makes a distinct contribution to our understanding of Cossack life and institutions under the last three tsars, Alexander II, Alexander III, and Nicholas II. And, if his thesis of a dual Cossack tradition is not entirely novel, it is presented in a more elaborate and convincing fashion than in any previous account. McNeal is not a stylish writer, his prose being workmanlike rather than elegant. But he is lucid, balanced, and accurate, and he organizes a wide range of material—on Cossack administration and finances, training and equipment, ceremonies and rituals, among other important topics—in an admirably coherent form. His book will be read with profit by all students of imperial Russia during its declining decades.

PAUL AVRICH
Queens College
City University of New York

BERNICE GLATZER ROSENTHAL, editor. *Nietzsche in Russia*. Princeton: Princeton University Press. 1986. Pp. xvi, 424. Cloth $50.00, paper $19.95.

This book is a collection of essays presented (with three exceptions) at a conference held at Fordham University in June 1983. The intent of the gathering was to examine the influence of Friedrich Nietzsche on Russian intellectual and cultural life between 1890 and the early years of the Soviet era. As George Kline notes in his foreword, Russians received Nietzsche with fascination mixed with shock and aversion. But there was more to it than that, of course, and the book proves to be a constantly interesting and stimulating account of the numerous ways in which Nietzsche and his ideas influenced Russia.

The book is divided into four main parts, each focusing on a major area of Nietzsche's potential influence. The first section, following Bernice Glatzer Rosenthal's very competent introduction, deals with the impact of Nietzscheanism on Russian religious thought and makes some of the strongest arguments for Nietzsche's influence. Ann Lane traces Nietzsche's "arrival" in Russia in the 1890s, emphasizing the generally positive response to him by Russian intellectuals at the end of the decade. Rosenthal's essay on D. S. Merezhkovsky discusses the intellectual dialogue the novelist carried on with Nietzsche's ideas. It was a dialogue that evolved from Merezhkovsky's "Nietzschean tract," *Julian the Apostate*, to the "new religious consciousness" of the early 1900s (a response to Nietzsche's attacks on Christianity), to a rejection of Nietzscheanism by 1908. Anna Crone argues for a strong Nietzschean influence on Vasily Rozanov and makes her case convincingly, although Rozanov himself consistently denied any influence. Taras Zakydalsky restores to us the forgotten criticisms of Nietzsche written by the little-known "outsider" N. F. Fedorov, for whom Nietzsche was the most interesting major thinker of his time. This first section of the book concludes with Mihajlo Mihajlov's essay characterizing Nietzsche as the "great catalyzer" in early twentieth-century Russian thought. Mihajlov contends (in some disagreement with what most of the other contributors say) that Nietzsche's ideas only stimulated the development of native Russian ideas already present in the Russian intellectual tradition (expressed by Dostoevsky particularly). Russian spiritual, religious, and mystical trends that Nietzsche would have rejected were actually revivified by Nietzsche's negative influence, Mihajlov believes.

The book's second part is concerned with the always fertile field of Russian symbolism and considers Nietzsche's influence on a number of Russian symbolist writers, artists, and musicians. Evelyn Bristol discusses the difficulties Aleksandr Blok had in reconciling Nietzsche and Vladimir Soloviev and argues effectively that whereas Soloviev's ideas were of primary importance to Blok early in his career by its end he had adopted almost entirely a Nietzschean frame of mind. Virginia Bennett makes a somewhat mechanical search for Nietzschean ideas in Andrei Belyi's essays from the middle of the first decade of the century, although she notes (in support of Mihajlov's position) that one reason Belyi valued Nietzsche was that he supported and confirmed Belyi's own views on the role of symbolism in formulating the aesthetics of the future. George Kalbouss takes on the unpromising task of uncovering Nietzschean traits in Fedor Sologub, the most "Schopenhauerian" of Russian symbolists, and in the end finds little of substance to say about Sologub's "Nietzscheanism." Lane discusses, with more success, Konstantin Balmont and Aleksandr Scriabin, the "artists as supermen" whose aim was to destroy old worlds and create new ones. Aline Izdebsky-Pritchard is less convincing about Nietzsche's impact on Mikhail Vrubel and admits that, although Nietzsche acted as a catalyst (again) on Vrubel, there is only "fragmentary [documen-

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tary] evidence" of Nietzsche's direct influence on one of the most individualistic of Russian artists.

The third section of the book is devoted to the pregnant topic of Nietzsche's impact on Russian Marxism. Mary Loe's article on Maxim Gorky's Nietzscheanism is certainly one of the best contributions to the book. Tracing the quest for a Russian superman by a writer seen by many critics of the 1890s as a Nietzschean, Loe argues that Nietzsche's influence on Gorky was dominant and lasting and that it has been seriously underestimated by both Soviet and Western scholars. Her essay is so stimulating, in fact, that the other two in the section suffer by comparison. A. L. Tait writes on A. V. Lunacharsky as a Nietzschean Marxist but never seems to be able to construct a fully satisfying picture of Lunacharsky himself, probably because of the limited space he has available. In an article on A. A. Bogdanov, Zenovia Sochor argues that Bogdanov's search for cultural liberation tied him to Nietzscheanism but admits that the contrasts between the two were great and concludes that in the end there were many more differences than similarities between the two of them.

The book's final section is something of a catchall. Richard Davies has put together a useful chronological bibliography of writings by and about Nietzsche published in Russian. James Curtis's essay on Mikhail Bakhtin's debt to Nietzsche fits rather awkwardly into the collection as a whole because of Bakhtin's own peculiar position in Russian intellectual life. Finally, Edith Clowes contributes an excellent essay on how Nietzsche's idea of the superman was vulgarized in Russian popular fiction by writers such as Leonid Andreev and Mikhail Artsybashev. She concludes that it was this vulgarization that damaged Nietzsche's reputation in Russia more than anything else did and led to the turning away from Nietzscheanism that became a stampede by the immediate prewar years. The book concludes with a brief afterword by Susan Ray concerning the limited but surprisingly positive comments Nietzsche himself made about Russia and its inhabitants.

Interdisciplinary collections of essays are not always coherent because of differences of style, methodological approach, or types of analysis. That is not the case with this book, however, which hangs together well and succeeds in its goal of being the first substantial study of Nietzsche's influence in Russia. If its contributors at times ask irrelevant questions in pursuit of their topic or express their enthusiasms for the subject too expansively, they do provide the reader with much information that is new or newly considered, and all the essays in the collection encourage a stimulating intellectual exchange between reader and author. The book proves too that despite the numerous studies of Russian Silver Age and revolutionary culture published in recent years many important issues still need to be addressed. This book has helped us begin to understand one of those significant topics.

WILLIAM RICHARDSON
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WILLIAM RICHARDSON. Zolotoe Runo *and Russian Modernism: 1905–1910*. Ann Arbor, Mich.: Ardis. 1986. Pp. 231. $27.50.

From 1906 to 1909 the major Russian symbolist journals were *Zolotoe Runo* (The Golden Fleece) and *Vesy* (The Scales). William Richardson maintains that *Zolotoe Runo* "is the principal contemporary document for helping us understand the peculiar nature of the Russian *fin de siècle* ... and the peculiarly Russian path away from 'decadence' and toward a kind of modernist populism" (p. 33). His study describes its organization, content, editorial policy, and activities in the context of the changing Russian cultural climate of those years.

Part 2 (the first main section), "Formation, Milieu, and Crisis: *Zolotoe Runo*'s First Two Years of Publication," is a detailed description of the journal's appearance, reception, organization, and content up to mid-1907. Financed by the millionaire industrialist Nikolai Riabushinsky, it had an opulent appearance, including gold letters and elaborate art work. Its editorial policy was to promote modernism and enlighten the educated public. The art section featured illustrated articles on the symbolist painters Mikhail Vrubel and Viktor Borisov-Musaget, pictorial reviews of Russian art exhibits, articles on special topics such as eighteenth-century Russian embroidery, and essays, some of them quite controversial. The literary section featured modernist poetry and literature, including translations of foreign works. The music section included theoretical essays and critical reviews by Vyacheslav Karatyin, Ilya Sats, K. Eiges, and "Vol'fing" (pseudonym of Emilii Medtner). *Zolotoe Runo* did not define its own identity vis à vis competing journals, nor did it stake out a distinctive aesthetic or political program. Publishing divergent views, the journal displayed an eclecticism and editorial tolerance that proved to be a problem in attracting subscribers and led to its crisis in mid-1907 and subsequent redirection.

Part 3 (the second main section), "Polemics, Neo-Primitivism, and Decline: *Zolotoe Runo*'s Final Two Years of Publication," describes aspects of the redirected journal. Seeking connections with the people, some writers tried to revive Russian artistic traditions; others advocated "realistic symbolism," a vague term that included the incorporation in art of national elements comprehensible to the people. Treated in detail are Aleksandr Blok's essays for *Zolotoe Runo*, its discussions of "mystical anarchism"

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and "realistic symbolism," further polemics with *Vesy*, the important artistic exhibitions it sponsored, other publication activities, foreign influences, and the reasons for its demise. Despite *Zolotoe Runo*'s heightened interest in the national question in art and literature, it continued to publish foreign painting and literature, including translations of Van Gogh's letters and admiring critiques of the work of Gauguin and Matisse. Though not uncritical of *Zolotoe Runo*, Richardson emphasizes its positive role as a "cultural catalyst," arguing that it "provided economic and spiritual support for what would later become disparate groups, maintaining them at their most vulnerable moments. By 1910 the artistic and literary tendencies aided by *Zolotoe Runo* no longer needed its help" (p. 177).

This study provides valuable information on a major journal and important period of Russian culture. Although some of the material has been treated by other scholars, typically as part of broader thematic studies, Richardson's book enables us to view the journal as a whole and provides an essential perspective on how *Zolotoe Runo*'s origins, evolution, and demise mirror the cultural issues and problems of the time. Particularly suggestive are his comments on *Zolotoe Runo* as a source of the postsymbolist doctrines of acmeism, futurism, and primitivism. But the coherence of the book could be improved. Its division into two main sections, the first organized chronologically and the second thematically, leads to a certain amount of repetition and overlap, which can be confusing to those not already familiar with the subject. The personal backdrop to many of the disputes, especially "mystical anarchism," is somewhat understated, and exactly what "realistic symbolism" means is not clear. The redirection in mid-1907 is somewhat exaggerated; the symbolist search for connections to the people began earlier; indeed one could argue that it began in September 1905 with Vyacheslav Ivanov's article, "The Crisis of Individualism." Richardson's discussion of the peculiarities of the Russian *fin de siècle* would have benefited from a comparison with William McGrath's *Dionysian Art and Populist Politics in Austria* (1974) and with other books and articles that appeared since the dissertation was written (the bibliography was not updated for the book) and from a more extended treatment of the national question as an aspect of this peculiarity.

BERNICE GLATZER ROSENTHAL
Fordham University

JANE BURBANK. *Intelligentsia and Revolution: Russian Views of Bolshevism, 1917–1922*. New York: Oxford University Press. 1986. Pp. viii, 340. $32.50.

This study examines the reactions of selected Russian intellectuals of the political center and Left to the revolution and the early stages of Bolshevik rule. Jane Burbank begins with Mensheviks Iulii Martov, Pavel Axelrod, and Vera Zasulich then proceeds to discuss the writings of "populists" Viktor Chernov, Mark Vishniak, and Petr Kropotkin, liberals Pavel Miliukov and Petr Struve, and monarchists F. V. Vinberg and S. S. Oldenburg. She concludes with a chapter on several politically unclassifiable but interesting writers—Nikolai Berdaev, N. S. Trubetskoi, and N. V. Ustrialov.

It is difficult to find shared ideas here, save fervent dislike of the Bolsheviks. Yet Burbank uncovers a commonality these men themselves would be loath to acknowledge: the tradition of social criticism and political disengagement that had characterized the Russian intelligentsia for generations. Although these men were alarmed by the disintegration of Russian society brought by the revolution, none was able to act effectively in 1917 to create a new political system. After the Bolsheviks seized control, these *intelligenty* returned easily to the role of disloyal opposition they had played under the tsar, holding onto their hopes of a popular overthrow of the new government while at the same time giving very marked evidence of nervousness about the people's capacity for self-rule. In no one is this ambivalence more profound than in Martov, who blathered about the bourgeois phase of history but opposed any collaboration with the bourgeoisie and who condemned Bolshevik intolerance but read people who disagreed with him out of the Menshevik movement. Plowing through Martov's confusion, one gains a new appreciation for Lenin and Trotsky.

Burbank has done an impressive amount of research, both in printed sources and in archives in Western Europe. Her writing is crisp and clear, her presentation of the ideas of these men lucid. One wishes occasionally for stronger integrative analysis, however. There are lengthy discussions of works that have little lasting value as commentaries on the revolution, being written far too close to the events. The significance of such ephemera lies in what they reveal about the thought of their authors and about the history of the intelligentsia. Martov, for instance, seems so muddled that one begins to understand why Trotsky consigned him to the dustbin of history. The contradictions in his thought need to be fully identified and, if possible, explained. So too do the roots of all these men's ideas in the Russian and West European intellectual traditions. The deeply Orthodox spirit of Kropotkin's faith in the Russian people and the Spenserian and Burkean influences on Struve's bitter condemnations of those same people would be more intelligible if more clearly labeled. A deepening of the criticism of each man's

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ideas would in turn produce a more thorough discussion of the intelligentsia as a whole. Nationalism, for example, affected all these men, even the leftists who vehemently denounced it. Examining nationalism and the other currents of belief within the intelligentsia would enhance our understanding of the ways in which Russia's social critics responded to the revolution.

Burbank concludes her work with a strong summary chapter in which she discusses such general questions as these with mastery. The book would be stronger if this analysis pervaded and integrated the whole. Nonetheless, this is a stimulating study that evokes powerfully the strengths, but mostly the weaknesses, of those Russian intellectuals left on the sidelines by the forward rush of revolution.

BARBARA EVANS CLEMENTS
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MIKHAIL HELLER and ALEKSANDR M. NEKRICH. *Utopia in Power: The History of the Soviet Union from 1917 to the Present.* Translated by PHYLLIS B. CARLOS. New York: Summit. 1986. Pp. 877. $24.95.

In recent years Soviet history has become the hottest subject in Slavic studies. Graduate students rush to write dissertations on post-1917 topics, established scholars of imperial Russia enter the field, new courses are taught to packed audiences. Even the Soviets have been cooperating by making a limited amount of archival material available to researchers. These developments have spawned several new textbooks or syntheses of Soviet history meant to present the fruits of recent research and to tackle again the major historiographical and moral issues of the field. Mikhail Heller and Aleksandr M. Nekrich here offer Soviet history seen through the eyes of two gifted émigré intellectuals. This massive volume, which originally appeared in 1982 in Russian and French editions, is the most detailed of the new syntheses to appear and also the most polemical. It is extremely well written and compelling. It conveys the excitement and moral urgency of the authors and their subject. It is also frustrating, repetitive, and not always judicious in its historical judgments.

The authors take a hostile stance toward the Soviet Union and its history: "The history of the Soviet Union is one of a society and state subjugated to a party, that of a state which has enslaved society, and that of a party which seized state power in order to create a human type that would allow it to keep power forever. It is also, however, a history of eloquent human resistance to that enslavement" (p. 732). They also attack Western historians who find the roots of the Soviet experience in Russian or Muscovite history, social structures, and political culture. The authors' model is monocausal—the party and its utopian ideology dwarf all other factors and act "as a determining force in all Soviet institutions and on the typical Soviet citizen, Homo Sovieticus. This total influence has distorted the normal processes at work in contemporary societies and has resulted in the emergence of a historically unprecedented society and state" (p. 11). The authors never depart very far from this agenda with its condemnatory tone. If the party has caused all the problems we should ask: What is the party? Out of what social and cultural material was it created? To these questions the book offers few answers.

Readers will not be surprised at the implications of all this for our relationship to the Soviet Union today. Detente of the 1970s becomes a moral and political fiasco, arms control an impossibility, and reforms an unthinkable and discordant element within the authors' totalitarian framework. Most of this is standard criticism of the Soviet Union and will raise few eyebrows. And, to be sure, the authors argue with real élan and coherence given their basic assumptions. The persistent comparisons of the Soviet Union to Nazi Germany are, however, disturbing. This *valid* question is never posed or answered systematically. Instead, comparisons are made with the "facts" supposedly speaking for themselves. We are left with an impression of equivalence. Collectivization, for example, is the "first socialist genocide." Although there remains doubt about how much the German population knew about the destruction of the Jews, Heller and Nekrich proclaim that "there is no doubt that the Soviet city people knew about the massacre in the countryside" (p. 236). Moral outrage replaces explanation as we learn little about the agrarian issue itself. Comparisons to the Nazis are plentiful in the chapters on the 1930s and World War II.

The book opens with a chapter on 1917 that portrays the October revolution as the work of Lenin and his utopian ideology. The authors counter Soviet claims that the insurrection was well planned. They note party opposition to Lenin's positions but say nothing about the scale and nature of mass support for a "Soviet" seizure of power, the already existing infrastructure so crucial to Soviet state building, or the role of peasants, workers, and the lower middle class. In a book that provides marvelous vignettes and hard data on Soviet society, it is striking that key interpretations are often based on narrow political analysis or the role of personalities. The October revolution was all Lenin's work, although the authors muse on his whereabouts between October 20 and 24. Lenin's values were identical to Stalin's. Stalin did not create the apparatus but merely developed and exploited it. Trotsky's defeat in the 1920s was the result of his

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own antidemocratic nature, his myopia, and his Marxist beliefs.

The New Economic Policy (NEP) is well portrayed as a mixture of utopianism and realism. The authors admit NEP's accomplishments but also note that the population lived amid "fear and uncertainty." The authors are fond of S. Dmitrievsky's portrayal of Stalin as a national Bolshevik, the creator of a national people's dictatorship that was superior to both democracy and socialism. Dmitrievsky argued that Stalin's system "was far more closely linked with the masses than any so-called democracy." This is a telling point that deserves more emphasis in the book. It is not enough to state that "a Stalinist state needs a Stalinist society." The social history of "Stalinism" requires a systematic analysis of social groups and their cultures. The Party Congress of 1934 (the "Congress of Victors") is compared to a similar Nazi rally at Nuremburg that year. The authors interpret S. M. Kirov's assassination in terms of Stalin's own designs on the party. Kirov, they argue, was hardly a more liberal politician than Stalin, and he was incapable of producing an opposition program as has been argued by Soviet and Western historians. Stalin ordered the assassination to attack the party itself, to create enough insecurity, fear, and internal strife to justify calling in the state security apparatus to resolve the tensions and save the state. The purges flowed naturally from this motive. Here the authors emphasize the large role of the NKVD and prisoner labor in the immediate prewar economy.

Surprisingly, and without convincing evidence, the authors argue that Stalin wanted a pact with Germany as early as 1934. Stalin desired to return to a traditional German orientation, and he recognized that it was in the Soviet Union's interest to promote conflict among the capitalist nations. The nonaggression pact of 1939 was the culmination of many years of conscious policy and not dictated by Western weakness or the German threat. In addition to Soviet territorial gains, Stalin, the authors argue, was motivated by a desire for a European conflict that could only weaken capitalist enemies while allowing the Soviet Union time to prepare for war. The coverage of World War II is full of fascinating details on military campaigns and on the home front and horrifying material on the fate of prisoners, nationalities, and the general population.

The judgments in the book are confident and assertive. Stalin and his generals were incompetent military leaders during the early months of the war. Yalta was "the blessing of the Soviet empire." The authors admit, however, that, given the actual military situation and pro-Soviet sentiment and domestic politics in the West, probably nothing else could have been achieved. There is deeper coverage of A. A. Vlasov's army and collaboration, nationality resistance, repatriation, and the methods of integrating new parts of the empire than of the evolution of the cold war. The section on post–World War II society and economy is particularly strong. A decline in wages and rise in the cost of living defined the immediate postwar period. Apparent declines in prices during the Stalin era were an illusion. Finally, mature, or post–World War II, Stalinism is described, but its characteristics (arbitrariness, police control, tension among the nationalities, and chauvinism including anti-Semitism) are hardly distinguishable from descriptions of the system in its youth.

The final chapters, though disorganized and too discursive, contain much important detail on the nationalities, cultural developments, and general domestic and foreign policy. The treatment of culture here and throughout is uniquely informed by the authors' experiences as members of the intelligentsia. They portray Khrushchev as a reformer sincerely interested in improving peoples' lives, as a man of contradictions including democratic tendencies, but also as the promoter of pernicious forms of social control that brought the era of the new Soviet man even closer to fruition. Brezhnev is the symbol of collective leadership gone sour—conformism, stagnation, corruption. Andropov is authoritarian, Chernenko inept. If you accept the premises of this book, expect little from Gorbachev. Heller and Nekrich eloquently develop another thesis, perhaps the most important in this book, about the emergence of public opinion and conscience in the Soviet Union. Despite the distortions in Soviet society and the party's goal of altering or destroying collective memory, resistance to the system is documented everywhere from the labor camps and workplaces to the kitchens of the intelligentsia and the borderlands. For Heller and Nekrich the diversity and moral power of dissent are central to post-Stalin society. They are the antidote to conformism, cynicism, and careerism, attitudes fostered even by so-called liberals in Soviet society who support dissidents in a variety of nonthreatening ways. The authors believe history must be reclaimed and that historical knowledge is a vital weapon against the excesses they describe. The details are striking. In no other text can we learn the names of the protesters arrested on Red Square during the invasion of Czechoslovakia in 1968 or so much about such other protests as the Kengir revolt, Mayakovsky Square, and the Novocherkassk strike. And this ultimately is the reason the book is so compelling. Whether or not the reader agrees with the arguments, there is new and exciting information and a moral claim that students of Soviet affairs and, eventually, Soviet citizens must resolve.

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Southern Methodist University

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CATHERINE ANDREYEV. *Vlasov and the Russian Liberation Movement: Soviet Reality and Emigré Theories.* (Soviet and East European Studies.) New York: Cambridge University Press. 1987. Pp. xiv, 251. $34.50.

Anyone who realizes that since World War II at least two hundred books and articles have dealt directly with the movement associated with Andrei Vlasov might wonder what could now be added. But, just as Joachim Hoffmann has recently shed new light on the military side of this topic, so now Catherine Andreyev has made a significant contribution on the political side.

Andreyev has used a wide range of primary and secondary published materials, plus more than a dozen archival collections. And she has interviewed over thirty former participants—an effort we can especially appreciate since time is fast thinning their ranks. Her book includes a comprehensive and judicious survey of what others have done, full citations to sources, and an extensive bibliography. The writing is clear, graceful, and precise. Andreyev dedicates the book to the memory of her father, without explaining to the uninitiated that he was the "N. E. Andreyev" listed among her interviewees and furnishers of archival materials: the noted Cambridge authority on Muscovite Russia, Nikolay Efremych Andreyev (1908–82).

Her main aim is to synthesize and comment on the political ideas of the Russians and others associated with what she properly calls not simply the "Vlasov movement" but the Russian Liberation Movement (RLM, in Russian, *Russkoe osvoboditel'noe dvizhenie*). She sees these ideas as the closest approximation we have to what educated Soviet citizens who lived through the 1930s under Stalin might have agreed on politically if they had been able to exchange opinions freely. At the same time she evaluates the other influences on the movement, including the NTS members and other Russian émigrés as well as the various kinds of Germans with whom the RLM had to deal. She discusses all the familiar documents expressing RLM views and appends the full texts of three of them. She also treats one that has been neglected: the *Bloknot Propagandista*, a publication of January 1945 designed for persons who were to teach and promote free discussion of the ideals of the RLM among Soviet citizens who were under German control. She obtained this publication from its principal author, one of her interviewees, whom she identifies incompletely as D. D. Galkin—following here as elsewhere throughout the book that incomprehensible and regrettable practice by which some publishers dictate that given names, even for important figures, be hidden behind mere initials.

The combined picture that emerges is of a political program that was basically democratic and anti-Stalinist, that differed significantly from the views of many of the sponsoring Germans and of the NTS and other earlier émigrés, and that was of necessity still far from fully formulated when the German surrender and the Allied-enforced repatriation abruptly closed this episode.

Andreyev, like Hoffmann, observes that the continuing importance of the ideas she discusses is shown by, among other things, the persistent and strenuous attempts of Soviet authorities to suppress or distort all references to the movement, Vlasov, and kindred subjects. How Soviet writers are permitted to treat the topic of this book will indeed continue to be one good way to measure *glasnost'*.

RALPH T. FISHER, JR.
University of Illinois

NEAR EAST

HABIB BEN ABDALLAH. *De l'iqta' étatique a l'iqta' militaire: Translation économique et changements sociaux à Baghdad, 247–447 de l'Hégire, 861–1055 ap. J.* (Studia Historica Upsaliensia, number 142.) Uppsala, Sweden: Uppsala University; distributed by Almqvist and Wiksell, Stockholm. 1986. Pp. 215. 140 KR.

Economic and social change are easier to describe than to explain. To accomplish the latter, Habib Ben Abdallah applies a clearly defined mode of production analysis to Baghdad and its environs. The key is the gradual change from the control of agricultural production by a centralized, civilian bureaucracy (*iqtā'* étatique) to military control (*iqtā'* militaire) from the mid-ninth to the mid-eleventh century. Since both systems are considered feudal, this change was a natural evolution from state feudalism to military feudalism. This is explained by the ineffectiveness of state *iqtā'* in ensuring a sufficient surplus through land taxes because of natural disasters, epidemics, famines, wars, and revolts that reduced agricultural production and land under cultivation. As taxes increased without increased agricultural production, officials resorted to extortionate methods of collection, while farmers resorted to "protection" by some powerful person or fled to cities. This was clearly more than a mere fiscal crisis, but the ineffectiveness of this system was aggravated by inflation and by the alienation of resources through concessions of tax-free land, granting lower (*ushr*) tax status to land, and keeping the income from extensive crown lands separate from state expenses. Ben Abdallah uses *iqtā'* in a generic sense for all forms of absentee landholding

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rather than in the technical sense of the rights to land taxes from a particular district. As the caliphal state collapsed, power passed from civilians to the military, who restored order, ended political and economic anarchy, and joined the urban landholding class by acquiring control over property through *iqtā'*, protection, and purchase. Thus, Ben Abdallah argues, the military nature of *iqtā'* was an effect of the change. But, since the military landlords were urban, it is difficult to accept his description of military *iqtā'* as decentralized, at least before the Seljuk period. Further, his claim that the majority of free farmers were reduced to peasants through protection lacks statistical support. However, he upholds the usual view that military *iqtā'* of usufruct expanded significantly under the Buwayhīs at the expense of civilian control and was consolidated by the Seljuks.

Ben Abdallah connects the militarization of landholding to socioeconomic changes in Baghdad. He calls the artisan sector the "second face" of *iqtā'* and argues that, although artisans owned their means of production, control by the *muhtasib* prevented them from organizing or producing freely, that specialization of markets and occupations was for easier surveillance, and that conflict among the people of different marketplaces and professions ensured political domination by the rulers and kept the commercial and artisanal sectors divided. Commercial capital itself was tied to landed property. He defines an Islamic feudal mode of production as the combination of artisanal units of production in cities with *iqtā'* in the countryside. He argues that stratification in Baghdad between dominant, nonproductive owners and dominated, productive nonowners, aggravated by the presence of dispossessed farmers and the landlords, provoked explosive class conflict over the possession and control of production in the countryside and over the distribution of production in the city. Whereas others have seen the social conflicts in Baghdad in this period as incohate, irrational, and ideological, Ben Abdallah sees them as rational and purposive and divides them into two phases: from 861 to 945 urban risings were directed against the military; from 945 to 1055 they were aimed at the merchants and financiers and destroyed the merchant economy. Minimizing the sectarian nature of these conflicts, he argues that poor Sunnis attacked rich Shi'is and vice versa. But to lopsided distribution as a cause of these disorders one could add a growing imbalance between population and the food supply resulting from changes in the countryside.

This multifaceted, sober, and sensible treatment of these subjects well integrates theory and evidence. Apart from Ben Abdallah's preference for evolutionary schemes, his translation of *ayyarūn* as "la masse," his conviction that Ismā'ilī-Qarmatī propaganda was the "latent ideological motif" behind these urban risings, which lacks evidence and should be balanced by Hanbalī attitudes, and the lack of an index, this is the most successful application of mode of production analysis to these issues thus far.

MICHAEL G. MORONY
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CORNELL H. FLEISCHER. *Bureaucrat and Intellectual in the Ottoman Empire: The Historian Mustafa Âli (1541–1600).* (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1986. Pp. xviii, 363. $49.50.

The historian Mustafa Âli of Gallipoli (1541–1600) is the bureaucrat and intellectual in the title of Cornell H. Fleischer's excellent book, which is at once a biography (part 1), an essay on the development of Ottoman bureaucracy (part 2), and a study of Ottoman historiography in the second half of the sixteenth century (part 3). These three parts form widening concentric circles around the central figure—a complex and difficult man of many parts, an intellectual of high stature yet at times also a sycophantic careerist. This penetrating and nuanced study rises to the demands of its subject; Fleischer's sympathy toward Âli is tempered with a fully critical and discerning appreciation.

At the level of biography this is the drama of an intellectual of the highest order consumed with ambition and therefore caught in the need to ingratiate himself with those in power, men he knows are inferior to him in intellect, knowledge, and ability. Fleischer traces Âli's career in a constant interplay between the individual and the institutional, showing what the stations meant in the Ottoman cultural and official context. The confident, able young man with an excellent education gained at the flourishing colleges of Istanbul soon abandoned the learned career (*ilmiye*) for service in grandee households and in the bureaucracy, but, although he wanted to rise on the basis of accomplishment and learning, he constantly needed to seek the protection of great personages. The aspirations of ambitious youth gradually gave way to the predicament of an able and learned man wasting away at insignificant provincial postings in middle age. Through changes on the imperial throne and the vezirate, through the waxing and waning of the fortunes of potential patrons, Âli hoped for high office befitting his intellectual and professional stature, yet the offices that came his way always fell short of his expectations of power and recognition.

As Âli the bureaucrat reached disillusioned old age, Âli the social commentator grew more disaf-

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fected with his milieu. Toward the end of his life, however, Âli the historian finally transcended his own professional predicament and mere observations on the current ills of the Ottoman state when he wrote his four-volume magnum opus, the *Essence of History*. In this universal history in the highest tradition of Islamic civilization, Âli came to grips with the contradictions of his own, Ottoman-Islamic, world. The issues he raised are universal and still relevant to modern historians and observers of Islam: the relation between Ottoman state law (*kānūn*) and the Islamic way (Shariat), the nature and sources of just rule and legitimate government, the particular Ottoman character based on racial and cultural Rūmi heterogeneity within the general context of the Islamic world sharing a religious and social heritage as well as the post-Mongol political tradition, the strength of Ottoman institutions and justice as the basis of polity firmer than ideology and imperial descent in Safavi Iran and Tīmūrid Mogul India. Âli the Islamic intellectual provided an explanation and justification of the practical Ottoman way, resolving dilemmas inherent in Ottoman political culture just as he overcame his personal predicament. It is curious, incidentally, that in his historical comparisons Âli does not seem to have raised the Shi'i identity of the Safavi empire nor detected a Mogul similarity to Ottoman polity in terms of governing large numbers of non-Muslim subjects.

Fleischer succeeds because he has not just written a biography but balanced his subject against the important developments of his times, so that the three parts of the book not only expand the topic but also reinforce each other. To do this Fleischer has digested all of Âli's own works, poetry and belles-lettres as well as historical prose (he has even discovered a few long-missing ones in the process) both for the biographical clues they provide and for their larger themes—this is why he has been able to produce such a satisfying study. This is a book of thorough research and elegant presentation that will be of enormous benefit to Ottomanists and Islamists as well as to intellectual and institutional historians in general. Fleischer points out that one of Âli's contributions to Ottoman historiography was his addition of extensive biographical sketches to the narrative. How fitting that Âli himself has now been brought to the large audience he always felt was his due.

I. METIN KUNT
University of Cambridge

JOHN ROBERT BARNES. *An Introduction to Religious Foundations in the Ottoman Empire*. Leiden: E. J. Brill. 1986. Pp. xii, 184.

The waqf, or pious foundation, has been a major institution in Muslim societies for well over one thousand years, nurturing countless charitable and educational organizations and, occasionally, descendants of the benefactors. John Robert Barnes's book aims to fill the gap caused by the lack of monographic treatments in English of the changing status of the foundations. Although there is a brief treatment of the waqf in earlier times, the more original part of the volume is a set of discussions of the legal and administrative character of pious foundations in the Ottoman domains, especially in the nineteenth century. The work surveys, therefore, a more restricted area than the title implies.

It is a great virtue of the book that it is based not only on published Ottoman legal treatises, which often discuss ideal situations and the status of waqf as one might have wished it to be, but also on archival materials: a small number of Foreign Office consular reports and a much larger body of materials from the Prime Ministerial Archives in Istanbul. Although the reader does not have a clear idea of the amount of archival documentation the author has not been able to use, the research here presented offers a fresh view on a number of topics of interest to all students of Ottoman history, such as the manner in which the government dealt with the Bektashi dervish order's resources, the specific impact of the Tanzimat "reforms" on the running of provincial foundations, and the gathering of waqf administration into the hands of the central government in the later nineteenth century.

Since the Ottoman rulers' acts in the Tanzimat era are normally viewed as responses to Europe, it has been natural to study aspects of those responses that fit European patterns. This book, however, speaks directly to the impact of the Tanzimat on a classical Muslim institution, and, even though the treatment is restricted to a few topics, it is a good reminder of the importance of the waqf, and of its problems, throughout the Ottoman countryside.

RUDI PAUL LINDNER
University of Michigan

FRANCIS DESRAMAUT. *L'Orphelinat Jésus-Adolescent de Nazareth en Galilée: Au temps des Turcs, puis des Anglais (1896–1948)*. (Istituto Storico Salesiano, number 3.) Rome: LAS. 1986. Pp. 318.

This book tells the story of the orphanage "Jesus Adolescent" in Nazareth, called in Arabic Abouliatama Abu 'l-yatāma), or "Father of Orphans." The institution was founded in 1896 by the Italian priest Antonio Belloni, survived Turkish and British rule, and is today, under Israeli rule, a flourishing school providing education to some five hundred Arab

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children ages nine to eighteen, half of them Christian and half Muslim. It is recognized as a secondary school, and its graduates are admitted to Israeli institutions of higher learning.

Much of the history of the school, as presented by Francis Desramaut, is based on its archives, and especially on a manuscript chronicle preserved in them. To judge from the excerpts printed, these archives are a valuable source not only for the history of the school itself but also for that of Nazareth as a whole. During the crucial months of the Arab-Jewish struggle for the city in 1948, the orphanage served as a refuge for hundreds of Arabs from various places in the vicinity of Nazareth. Among them were men, women, and children from Ailout (Eilouth), a Muslim Arab village that suffered heavy losses during the war, and whose inhabitants, the book informs us, had for a long time the reputation among the Christians of Nazareth of "savagery and violence" (pp. 221–22).

Although occasional observations such as this throw light on the reaction of foreign Christian leaders to the behavior of the local population, the interest of the author is centered on the history and workings of the orphanage and school. The long concluding chapter, entitled "Everyday Life at Jesus-Adolescent" (pp. 222–85), provides interesting details about the changes in the educational system in the course of more than six decades. To begin with, all subjects were taught in French, and only the sermon was given, and the catechism taught, in Arabic. The avowed purpose was "to save the Jewish and Muslim 'schismatics'" (p. 229). With the passage of years and the increase in the number of pupils (from 18 in 1896 to 270 in 1936), most of whom were children of Catholic (Latin, Maronite, and Greek Melkite) Arab families, there was a gradual shift toward Arabic as the language of tuition. The places of many of the original Italian and French teacher-priests were taken by Arab priests. Not all the foreign priests were satisfied with this development. Thus, Father Auguste Crozes wrote in 1931: "At the age of thirty, I am reduced to relearning Arabic if I want to talk to my youngsters . . . Arabic—it is worse than Chinese. One cannot read it unless one already can speak it . . . But, Great God! What a language! Eight dentals, seven gutturals! There are five or six letters that my poor gullet refuses to articulate."

Since 1972 the orphanage has been headed by Italian priests and staffed by Arab teachers. The notices, the inscriptions in the classrooms, the schoolbooks, the instruction were all in Arabic, with a few technical subjects taught in Hebrew. Few of the pupils understood any English, even fewer French. "The culture taught has become, in the course of years, frankly Arab" the author remarks with a note of regret (p. 9).

RAPHAEL PATAI
Forest Hills, New York

ISRAEL GERSHONI and JAMES P. JANKOWSKI. *Egypt and the Arabs: The Search for Egyptian Nationhood, 1900–1930.* (Studies in Middle Eastern History.) New York: Oxford University Press, in cooperation with the Dayan Center and the Shiloah Institute for Middle Eastern and African Studies, Tel Aviv University, Tel Aviv. 1986. Pp. xviii, 346. $39.95.

This is a well-conceived and brilliantly executed study that presents a wealth of new information and penetrating insights on a familiar topic. Basing their study on British archival sources and an extensive reading of Egyptian books, journals, and newspapers, Israel Gershoni and James P. Jankowski present an in-depth analysis of the ideology and practice of Egyptian territorial nationalism from its inception in the late nineteenth century through its heyday in the 1920s.

In part 1 the authors review Egypt's loose ties with the Ottoman, Islamic, and Arab worlds in the last years of the nineteenth century. Although acknowledging the influence of Western thought and development on Egyptian nationalist ideas, particularly Hippolyte Adolphe Taine's concepts of race, milieu, and moment and Gustave Le Bon's ideas on milieu and race, they argue vigorously that the political consequences of World War I and the Egyptian "revolution" of 1919 were the major events in focusing the ideas and political aspirations of Egypt's political and intellectual leadership on the uniqueness of the land and people of the valley of the Nile.

Throughout this excellent study the authors emphasize indigenous forces (environment, history, culture) to explain the emergence of an Egyptian territorial nationalism and thus provide a balance to those who would see Egyptian nationalism as chiefly a product of Egypt's process of Westernization.

Part 2 is the core of this work. In six well-written chapters the authors examine the formation of the concept of Egyptian territorial nationalism and follow its evolution as the dominant ideology of the 1920s. These chapters analyze the broadest selection yet of works by Egypt's intellectual leaders of both the first and second rank and relate their ideas to a wide range of topics and political events. The authors point out the strong negative image these intellectuals had of the Arab and Islamic worlds as the Egyptianist concept evolved into pharaonicism. So strong was this concept during the 1920s that Egypt's secular intellectuals "wished to separate Egypt in virtually all dimensions—geography, history, culture, literature, even language—from its Arab

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and Muslim neighbors" (p. 221). Calls were made to create a national literature to popularize the concept of territorial nationalism in books, plays, poems, and school textbooks. The almost complete dominance of the concept of pharaonicism is symbolized by the mausoleum designed to resemble a pharaonic temple that was constructed for the nationalist hero and independent Egypt's first prime minister, Sa'd Zaghlul, following his death in 1927.

Part 3 deals in two brief but informative chapters with Egypt's intellectual and political relations with the Arab and "Eastern" worlds during the 1920s. These are important contributions to the authors' overall study of Egyptian nationalist ideology in the twentieth century, for they provide an intellectual bridge to the next volume that the authors are preparing on the Muslim and Arab orientations Egyptian nationalist thought took from the 1930s onward. They have laid a firm foundation for the companion study through the systematic, exhaustive, and penetrating analysis of Egypt's intellectual milieu of the 1920s.

DANIEL CRECELIUS
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CALVIN H. ALLEN, JR. *Oman: The Modernization of the Sultanate*. (Profiles: Nations of the Contemporary Middle East.) Boulder, Colo.: Westview, and Croom Helm, London. 1987. Pp. xiii, 154. $28.00.

Traditionally, Oman was an important center of Indian Ocean maritime and commercial life and the seat of the distinctive Ibāḍī version of Islamic and Arabian culture and society. But a century of decline occurred after the mid-nineteenth century, and, by the 1960s, Oman had become an isolated backwater and a mysterious hermit state little known even by scholars of the Middle East. Reemerging from obscurity in the 1970s, Oman has since become the subject of a diverse and growing body of scholarship.

Calvin H. Allen, Jr.'s, book is the initial attempt to digest this new knowledge and to integrate it into a comprehensive survey. His study treats complex environmental, socioeconomic, religious-intellectual, and economic issues, but its essential cohesion is supplied by its determinedly historical perspective. It consists of three sections: an opening chapter sketching Oman's geography and physical environment, its society and culture, and its traditional economy; a four-chapter summary of the country's history from ancient times until the coup of 1970 that brought its present ruler, Sultan Qabūs ibn Sa'īd, to power; and a four-chapter account of the wholesale modernization that has transformed Oman since 1970. A well-selected, valuable bibliographical essay supplements the narrative.

The strengths of this well-conceived, lucidly presented little volume are many, and its weaknesses are few. Indeed, virtually its only unconvincing part is its treatment of the initial sixty years of rule by the current Āl Bū Sa'īd dynasty after its establishment in the mid-eighteenth century. A more sophisticated presentation of this period might have resulted had the insights offered by Patricia Risso in her various writings—especially her *Oman and Muscat, An Early Modern History* (1986)—been consulted. Use of the term "pirates," a borrowing from British imperial sources, to refer to Oman's late eighteenth- and early nineteenth-century maritime rivals in the Persian Gulf is, in my judgment, especially misleading.

But, in the main, Allen gives us a book suited to scholar and general reader alike marked by sound generalizations and a firm sense of the major trends and turning points in Oman's history. Indeed, his treatment of twentieth-century Oman is outstanding, especially his summary of events in the country since 1970. Allen, unlike most other students of the period, recognizes the continuing influence of tribalism in Oman as well as the potential appeal of a religiously sanctioned, quasi-populist Ibāḍī imamate should the present absolutist sultanate falter.

Allen has succeeded admirably in his objective of providing an authoritative synthesis of Oman's history with special emphasis on the contemporary period. It stands as the only work of its type currently available.

ROBERT G. LANDEN
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AFRICA

STEPHEN G. BUNKER. *Peasants against the State: The Politics of Market Control in Bugisu, Uganda, 1900–1983*. Urbana: University of Illinois Press. 1987. Pp. 284. $27.50.

Stephen G. Bunker's study makes a significant contribution to twentieth-century Uganda's economic history and to the debate about African peasant societies and their role in development. Although the subtitle suggests a lengthy historical survey of Gisu coffee growers in Uganda, the main focus of the book is on the period since World War II when coffee marketing among the Gisu was cast within the Bugisu Cooperative Union.

The Gisu are the only Ugandan ethnic group to grow arabica coffee, and this has given them an important position in Uganda's export economy. Bunker chronicles, with considerable sensitivity and a great deal of repetitive argument, the struggle

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between the freeholding Gisu peasantry and the colonial and national states for market control. The Ugandan state was (and is) dependent on peasant production and sought to appropriate the surplus the peasants produced; Gisu peasants sought to control their own crops and to maximize their return on the land and labor they invested in coffee growing. Local leaders, whom Bunker identifies as belonging to three status groups at different periods of time (chiefs, civil servants, and politicians), mediated between the state and peasantry. Throughout the period covered by the study, peasants retained a powerful bargaining tool in dealing with the state, the exit option (that is, withdrawal from cash-crop production in favor of subsistence crops). The centralizing and heavy-handed attempts of the first and second Obote regimes and the Amin dictatorship to take control of the Gisu coffee industry finally brought about just such an exit option. The author's treatment of this period (1966–85) is particularly informative.

In addition to detailing the politics of Gisu coffee growers and their cooperative, the book makes a contribution to broader developmental and historiographical issues. It provides a strong and, on the whole, convincing attack on dependency theory and those scholars who have sought to apply it to East Africa. Bunker argues that both the colonial and national states were essentially weak. They could only control appropriation tenuously through intermediaries whose powers and status were "deeply imbedded in autochtonous local social organization" (p. 252).

From the point of view of the historian, however, the book has some serious defects. Foremost among these is the totally inadequate system of citations and references. Although the author obviously has relied heavily on oral interviews and archival material in constructing his account, little is provided the reader as to authorship and dates of letters, reports, and interviews. In the few instances that references are provided, they are often incorrectly cited (for example, unpublished reports become published in the text though not in the list of references at the conclusion of the book). Another significant weakness for me was Bunker's attempts to argue that class differentiation did not occur in Bugisu. He recognizes the emergence of a more wealthy group of coffee growers who were also employed and often engaged in business, but he maintains that these leaders' dependence on peasant support and their susceptibility to state intervention "prevented their emergence as a separate class" (p. 248). Yet the situation Bunker describes clearly indicates the presence of competing groups of petite bourgeoisie among the Gisu.

These criticisms aside, the book represents a most welcome contribution to our knowledge of Uganda's economic and political history, subjects about which there has been little scholarly publication over the past decade and one-half.

ROBERT M. MAXON
West Virginia University

DANE KENNEDY. *Islands of White: Settler Society and Culture in Kenya and Southern Rhodesia, 1890–1939.* (Duke University Center for International Studies.) Durham: Duke University Press. 1987. Pp. x, 271. $30.00.

This is mainly a psychological study of European settlers in Southern Rhodesia and Kenya from the beginnings of settlement to the eve of World War II. In the first half of the volume Dane Kennedy lays the foundation for his analysis by describing the origins of settlement and the related historical developments. Rhodesia and Kenya had much in common. Both were British dependencies that began under the rule of commercial companies, and they were situated in healthful, pleasant climates. Unlike Algeria and South Africa, however, they were remote and expensive to reach with the result that they did not attract poorer classes directly from Europe. In each territory the white settlers had to overcome stiff resistance from the indigenous inhabitants to establish their claim to the land. Although they exerted a strong political influence, moving the imperial power to govern primarily in their interest, they remained in the words of Rhodesia's prime minister, Sir Godfrey Huggins, only "an island of white in a sea of black" (p. 2). It was to a great degree the presence of the Africans in such great numbers, offering a bountiful supply of cheap labor, that made these countries so attractive.

Though similar in important fundamentals, Rhodesia and Kenya developed differently. In Rhodesia settlement preceded the railways as adventurers trekked from South Africa to search for gold and other minerals. Later, as farmers followed the prospectors, Rhodesia remained a frontier of South Africa while Kenya became mainly a frontier of India. Afrikaner immigrants constituted about 90 percent of Rhodesia's settlers but only 20 percent of those in Kenya. The presence in Kenya of numerous immigrants from South Asia, who soon comprised the majority of the traders, artisans, and clerical workers, precluded the immigration of middle- and lower-class Europeans and the rise of a European middle class. The initial settlement in Kenya took place along the railway as the government alienated large blocks of agricultural land. Kenya thus attracted reasonably well-to-do and often titled immigrants, mainly from Britain, whereas Rhodesia became an outlet for South Africa's poor whites. As Europeans in Kenya developed as a

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landed aristocracy, those in Rhodesia moved into trade and transport and became, for the most part, middle-class businessmen.

It was not the differences but the features they had in common that determined the culture of the two societies. The author's thesis, expounded in the second half of the volume, is that these two seemingly disparate societies developed a common culture. In both the settlers' "power was matched by fear, arrogance by anxiety, disdain by suspicion" (p. 187). They were fearful of both the climate and the Africans. For protection from the tropical sunlight, they devised special clothing and housing. Obsessed with the "black peril," they established complex systems of segregation, developed pidgin languages for communication with Africans, abhorred miscegenation and poor whitedom, and created the myth of a classless white society. The result in each territory was a profoundly conservative society bent on securing itself against all change.

Despite its portrayal of a pathological settler society, the study is objective and constitutes a valuable contribution to the history of British colonialism. It is based on an examination of private and official manuscripts in Zimbabwe, Kenya, and Britain, including, it seems, all the collected settler papers, together with the relevant periodicals and published literature. It is fully documented, and its arguments are persuasive. Unlike many works on colonialism, the volume appears to be written without prejudice and without reference to a preconceived view of determinants.

ROBERT G. GREGORY
Syracuse University

ANDREW PORTER. *Victorian Shipping, Business, and Imperial Policy: Donald Currie, the Castle Line, and Southern Africa*. New York: St. Martin's or Boydell, Woodbridge, U.K. for the Royal Historical Society. 1986. Pp. x, 332. $35.00.

Superbly researched and densely argued, this work examines several interrelated themes: business practice and capital formation, shipping trends, political-capitalist relations (in and between Britain and South Africa), and imperial activity. Two interrelated and paradoxical theses emerge. Although Donald Currie (1825–1909) amassed a personal fortune of 2.4 million pounds, many of his tactics are explained by his need to manage the sizable debt of his principal company, the Castle shipping line. Further, although he needed government aid (and assiduously lobbied for it) and government departments needed his company's services, their relations were often acrimonious and invariably tenuous.

Currie and the Castle line left meager evidence. Therefore, as Andrew Porter notes, an elaborate rendering of Currie the man and a full profit-and-loss reckoning of the company are not possible. But, through a remarkable foray into a diverse collection of government, business, and private archives, as well as periodicals in Britain and South Africa, Porter has uncovered much relevant material. All in all this is a prodigious work of scholarship.

Porter is as hard and unsentimental in evaluating the Nonconformist Scot as was Currie in the pursuit of economic advantage. That was not an end in itself for Currie, providing for himself and his family was. The needs of shareholders or imperial ventures in aid of a greater Britain were not priorities. Currie's strategies are critically examined against a background of what was happening in the shipping industry and the current phase of the business cycle. Often as not he moved into a part of the shipping trade as it was about to run into difficulty, or he ordered too many of the wrong kind of ships. More the creature of circumstance than an innovator, Currie used secrecy and heavy-handed management techniques to square disgruntled shareholders and to appease bankers who held his paper. He was, however, an innovator in one respect. He cultivated the press by staging events such as taking William Gladstone on a cruise on his poshest ship.

Since Currie's business career coincided with the era of the "new imperialism," since shipping pushed expansion, and since Currie was active in investment and speculation in South African diamond- and gold-mining enterprises, Porter assesses his contribution to the phenomenon. And he makes it abundantly clear that Currie was an imperialist by necessity and not conviction and that his leverage in dealing with British and South African governments and other shipping companies was always extremely limited. For example, in seeking mail contracts—all-important to the continued viability of his company—convincing one government department, say the Admiralty, was not a guarantee that the Colonial Office, or the Foreign Office or the Treasury, would also comply. Meanwhile, rivalry between colonial governments, such as that between Natal and the Cape, seemed only to provide temporary advantages to exploit. And, as his contribution to the creation of the South African shipping ring in 1883 and his amalgamation of the Castle line with a major competitor, the Union Steamship Company, in 1900 show, he never was able to get the kind of government backing that would have allowed him to beat his commercial adversaries. He had to join them. But if companies could get bigger so could governments. The union of the South African colonies in the year following his death was, paradoxically enough, partly because of and partly in defiance of his efforts. His shipping company had contributed to South Africa's economic growth. It

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was now strong enough to dictate terms to the shipping combination he had created.

Porter might have contributed more to the literature on imperialism by systematically showing how Currie's career and the institutions he created illuminate current historiography, particularly the radical genre that so much informs the writing of South African history. He prefers rather to set out Currie against contemporary critics. J. A. Hobson's excesses are neatly demolished. But others no doubt will use Porter's excellent study in the imperialism debate—one hopes with the extraordinary care he used in writing it.

ROBERT KUBICEK
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ASIA AND THE EAST

STANLEY WEINSTEIN. *Buddhism under the T'ang.* (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1987. Pp. xiii, 236. $39.50.

This is an excellent historical survey of imperial policies toward Buddhism during the T'ang dynasty (618–907 A.D.). Based on a rich array of Chinese and Japanese sources, this book is the most detailed narrative discussion of its topic in a Western language. It clarifies and orders material we have known before only in a less systematic way. Its eighty-five pages of notes, glossary, bibliography, and index provide a feast of information for future researchers in the field.

As Stanley Weinstein notes in the preface, this book began as a chapter in the *Cambridge History of China* "for readers whose interests were primarily in the history and institutions of T'ang China." It deals with "the secular side of T'ang Buddhist history" (p. ix). This is fine as far as it goes, but the result of course is not a history of the internal development of T'ang Buddhism, as the author recognizes. Buddhist schools, masters, and translation projects are mentioned only as they interacted with the court. Since the political history of Buddhism at the provincial and local level is not discussed, one cannot even say that this study deals fully with Buddhism and the state. A change in the social context of major Buddhist schools is noted with the rise of provincial warlords in the eighth century, but social history as such is not discussed. Nor are there discussions of texts, rituals, the activities of temples and monasteries, or relationships between clergy and laity. The self-understandings of Buddhism, its own internal dynamics, and why it persisted despite repeated suppressions remain in the background. Since the author's approach was consciously chosen, and he has done well what he set out to do, these comments may seem unfair but must be made nonetheless. There are important methodological issues at stake here, issues that shape an understanding of Chinese Buddhism.

The book is a case study in the relationships between a despotic state and a religious institution that sought to maintain some independence. One is impressed most of all with the extreme oscillations of imperial policy toward Buddhism, both between reigns and within the lifetimes of individual emperors. In 767, for example, the court approved the destruction of several official buildings in the capital so that their materials could be used to construct a monastery (pp. 83–84), whereas at other times new monasteries were forbidden and old ones destroyed. It all depended on the beliefs of the emperors and on the influence of their Confucian and Taoist advisers. There is material here for students of contemporary Chinese government policies; arbitrary shifts have been going on for a long time.

The most interesting discussions in this book are those of the progressive destruction of monastic Buddhism across China during the An Lu-shan rebellion of 755–63 and resulting from the restrictive edicts of Wen-tsung (reigned 826–40), of the wholesale suppression carried through by Wu-tsung (reigned 840–46), and of the ruin of thousands of monasteries and temples during the Huang Ch'ao rebellion of 875–84. In the process many scriptures and schools were destroyed, to the point that the Hua-yen and T'ien-t'ai tradition were later restored only with the help of Korean Buddhists.

As Weinstein notes, "With its transmissions shattered and crucial texts lost, the character of Chinese Buddhism underwent a fundamental transformation after the T'ang" (p. 149). He adds that the two schools that survived were the Pure Land and Ch'an (Zen), which depended much less on scriptures and exegesis. They also survived because they had a broader popular base, a base that made possible the Buddhist revival of the Sung (960–1279).

In sum this book is a fine updated statement of a long-established traditional approach to the history of Chinese Buddhism. Its clarification of the larger political context will help guide those who are studying other aspects of Buddhist life and thought.

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RICHARD J. SMITH. *China's Cultural Heritage: The Ch'ing Dynasty, 1644–1912.* Boulder, Colo.: Westview or Frances Pinter, London. 1983. Pp. xiv, 321. Cloth $28.50, paper $12.95.

This book is a fine survey, especially for pedagogical purposes, of Chinese middle to high culture during

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the last dynastic period. I recommend it as a text for upper-division undergraduate courses on "traditional" China, for providing "background" to instructional material on twentieth-century China, and as reading for any well-educated person who wishes to learn about enduring patterns in how the Chinese organize themselves and interpret society and the natural world.

Those who specialize in the study of the late imperial era need not brace themselves to endure the simple-minded, homogenizing, hackneyed generalizations that pervade many surveys of premodern Chinese culture. Admirably, Richard J. Smith, by focusing on the Ch'ing period, has managed to "say something important not only about the previous two thousand years of Chinese history but about modern China as well" (p. 1), without serving up the pablum that sticks in the throats of specialists. He has read widely and well in secondary literature (cited in careful footnotes and an ample bibliography), the findings of which he smoothly organizes to advance various themes that are basic to understanding Chinese culture but that seldom are presented so accessibly as in this book. Although I read little that was wholly new to me, I gained inspiration and many good ideas for conveying familiar material in better ways.

The author is more successful in pursuing the themes set out in his introduction than those in his preface. The latter begins with references to "the tension between the inherently divisive tendencies of a huge, geographically fragmented, and ethnically diverse land mass and the unifying impulses of a centralized bureaucratic empire" and to evidence of an "obsession with the concept of order" in Chinese "cultural psychology" (p. xiii). Treatment of these matters, however, is less thematically recurrent, and less satisfying or stimulating, than the three related themes outlined in the introduction, all of which have to do with "cognition, the way the Chinese viewed the world around them"; "complementary bipolarity" (especially as expressed in the "well-known, but much abused, concepts of *yin* and *yang*") as the basis of a "naturalistic world view"; an essentially nonreligious ethical system, the authoritarian tone of which was given concrete expression in the powerful symbols and metaphors of bureaucracy and family; and ritual, "including all forms of artificially structured social behavior," as a "'language' that celebrates man-made meaning" (pp. 3–6). Although breaking no new theoretical ground, the author succeeds in integrating these themes to give intelligent, though perhaps unfamiliar, readers "a clearer understanding not only of the internal 'logic' of the Chinese cultural system but also an appreciation for why it lasted so long and held together so well" (p. 7).

As one might expect, this book consciously emphasizes articulate thought and the cultural and governmental pursuits of the educated stratum in Ch'ing society. It is somewhat disappointing to find little therein about the military, about how ordinary Chinese people lived, or about the ground-level activities of production and trade that conditioned and supported the cultural forms that receive preponderant attention. Clearly, the author's interest lies mainly in the latter, but neglect of the former may also be a way of sidestepping difficult conceptual problems in relating socioeconomic conditions to mental and spiritual phenomena. Moreover, in the last chapter, "Tradition and Modernity," the author sets aside the more challenging question, which the rest of the book really leads up to, of how past patterns have become present patterns of thinking and behaving, that is, of how Chinese culture has evolved as a whole. Rather, his treatment restates observations based on dichotomous concepts of "internal" and "external," "traditional" and "modern," which are easily grasped by students but ultimately mislead.

Penciled question marks in the margins of my copy are few, and they appear mostly in the opening and closing chapters. For instance, in "The Ch'ing Inheritance" (chap. 2), I wonder whether the following statements are more slips of the pen than serious assertions: "premodern China did not possess anything like a single communications network" (p. 17); "there were few readily accessible and easily navigable north-south inland waterways in all of China . . . only the famous Grand Canal . . . provided long-distance water transport north to south" (p. 18); part of "the long-term legacy of the Shang [traditionally, 1766–1122 B.C.] was . . . a strongly bureaucratic outlook" (p. 21). That "a Han [206 B.C.–222 A.D.] scholar would have had very little difficulty adjusting to life in any subsequent dynasty up to the Ch'ing" is highly debatable (p. 24). Also, near the beginning of the final chapter, the author says, "I am persuaded that the reformist tradition of China's Confucian heritage provided an adequate intellectual foundation for modernization based on Western standards of . . . development." But his subsequent discussion does not support that view.

Such passages detract little, however, from a book that accomplishes worthwhile goals and will be found useful by many, both inside and outside academics. It is well produced, with arresting photographs and illustrations. I spotted only three or four typographical errors.

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JAMES H. COLE. *Shaohsing: Competition and Cooperation in Nineteenth-Century China.* (Monographs of the As-

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sociation for Asian Studies, number 44.) Tucson: University of Arizona Press. 1986. Pp. xv, 315. $21.00.

Part local history, part institutional history, James H. Cole's compact, insightful, well-written study examines the interaction of Shaoxing and China in the nineteenth century. He connects them through the phenomenon of Shaoxing's "disproportionate influence over the [Qing] dynasty's administrative system" (p. 3), an influence wielded by an extraordinary network of Shaoxing emigrants that comprised subcounty officials, central government clerks, and legal secretaries. Losers in the "law of the jungle" (p. 73) struggle for survival that was Shaoxing, these men achieved their success in the outside world through cooperation as Shaoxing "organization men."

Part 1's three chapters on local elites, respectable commoners, and outlaws and marginal elements covers the competition for social resources within Shaoxing. Drawing on G. William Skinner's core-periphery schema, Cole demonstrates that the nature of the conflict varied according to a county's place in the economic hierarchy, but, despite references at the outset of part 2 ("Survival through Cooperation") to population pressures and constricted examination quotas, precisely how the members of these groups are connected to the exodus of frustrated men of ambition remains unclear. Part 2 traces these men's movement into positions as subofficials (one of seven jail wardens in the empire was a Shaoxing man), as central government clerks in the Boards of Civil Office and Revenue whence they could facilitate their fellow natives' purchase of degrees and ranks and appointment to office, and as private secretaries (*muyu*) linked in a vertical alliance that ensured their employment by officials who wanted to make use of it. Clearly, this network constituted potential influence but to what end? Cole elucidates its mechanism but not its purpose. His concluding characterization of it as "centralism in the service of localism" (p. 153) lacks cogency because we do not see Shaoxing men cooperating beyond assisting each other either to implement a shared political view or even, as senior officials of the same native place were wont, to affect decisions concerning their home.

Although Cole argues that the secret of the Shaoxing success was organization rather than aptitude, he also intriguingly suggests that Shaoxing men drew on a heritage of legal knowledge, perhaps an intellectual legacy of Huang Zongxi that was strikingly encouraged in its transmission by lineages. This "amoral expertise" clashed with the Confucian distrust of the professional and emphasis on moral suasion and heightened the tension Shaoxing *muyu* felt as former clerks and would-be officials. Since not all Shaoxing subofficials, clerks, or *muyu* engaged in chicanery, this aura of professionalism may have contributed to their reputation as scheming shysters. Cole recounts little of the lurid, so we may not completely grasp why Lu Xun avoided admitting that he came from Shaoxing. Yet this book certainly begins to make clear how the men from this one prefecture became so pervasive.

JONATHAN K. OCKO
North Carolina State University

MARY BACKUS RANKIN. *Elite Activism and Political Transformation in China: Zhejiang Province, 1865–1911.* Stanford: Stanford University Press. 1986. Pp. xiv, 427. $39.50.

Here is a vital, major book that provides a broadly and solidly documented fresh analysis of the end of the Qing regime and the inauguration of the Chinese republic after 1911. Using evidence from the coastal province of Zhejiang, south of Shanghai and commercially tied to it, Mary Backus Rankin defines the role of an expanded social elite vaulted into local activism during the era of reconstruction after the Taiping Rebellion. Newly applying their instincts, talents, and resources to the funding and supervision of educational, popular welfare, and mercantile organizations, leaders at top layers of district and market-town societies managed the emergence of a "public" sphere involving networks of influence and action between "private" and "official." The legacy of such public activity was regional resistance to deliberate Qing state building in the last decade of the empire and mechanisms for oppositional politics that became the deciding factor in the revolution of 1911.

What new conditions in late nineteenth-century China turned a tradition of local-elite management into an unprecedented defense of local autonomy at the expense of the existing state? After all, the imperial Chinese state long depended on local nonbureaucratic energy and initiative; further, an eremitic scholarly tradition produced political critique. Rankin's case for novelty boils down to "increases in numbers and structural complexity" in public activity in the late Qing (p. 105). Such development is evident in the commercially prosperous, intellectually advanced province of Zhejiang, with its core area of five northern prefectures especially ripe for a coalescence of local activism and national policy.

In command of a wide range of sources, including local encyclopedic histories (gazetteers), Chinese and Japanese scholarship, and recent dissertations, Rankin illuminates a convincing progression. Reconstruction efforts in the 1870s secured the bond between merchants and gentry as a social elite, and

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this was true in both core and peripheral areas. The advent of the Chinese public press permitted the affairs of local social managers to have wider coverage and, conversely, to be part of extralocal movements such as north China famine relief in 1878. Local interests interacted with national policies of coping with foreign political and economic expansion and generated "participatory self-strengthening" (pp. 26, 154); events of the 1890s, including Japan's victory in the Sino-Japanese War, heightened political concerns. In 1907, when the Qing government let stand an agreement with a British firm to finance and construct a railway in Zhejiang against the wishes of local stockholders who had founded and funded the provincial railway company, there came into being a particular focus of oppositional politics. In the last years of the Qing, the decade of the New Policies, a politically mobilized managerial elite took advantage of the government's own efforts at constitutional rule to change the basis of China's state-societal relationship.

Rankin's work adds to and challenges some long-held views of the revolution of 1911 by putting that event in a broad context of social and conceptual change beginning in the 1860s and undermines the role of "revolutionaries" while also avoiding an explanation in terms of class. This is a bold book, with a felicitous and persuasive narrative that smooths newly mined, though uneven, data and fixes the period 1865 to 1911 as one of "political transformation" in the hands of a significant, expanding social segment of elite managers.

SUZANNE WILSON BARNETT
University of Puget Sound

JOHN KING FAIRBANK. *The Great Chinese Revolution, 1800–1985*. New York: Harper and Row. 1986. Pp. xi, 396. $20.95.

This marvelously readable synthesis incorporates the latest research findings in the history of nineteenth- and twentieth-century China, integrated by the judgments John King Fairbank has distilled from a lifetime study of China. Besides providing a clear narrative of events, Fairbank lucidly summarizes trends and patterns, defines basic issues, appraises leading individuals, outlines contrasting interpretations, and suggests myriad questions for further research. The book could serve splendidly as a text for courses on modern Chinese history.

Although the author has used recent insights and newly discovered information produced by a host of scholars, the underlying themes of the book are those Fairbank has been preaching during the past half-century. Those themes do not concern particular interpretations of this or that historical phenomenon, which are inevitably revised as research goes on (the inevitability of revision is, in fact, one of Fairbank's themes), as much as they emphasize the way we ought to think about the study of China.

Here, for example, as so often in the past, Fairbank emphasizes the high level of Chinese cultural, institutional, and intellectual achievements before the impact of Westernization, in order, as he once put it, to chasten "our Europocentric self-esteem." Fairbank argues that one cannot properly understand a people whom one views through the distorting lenses of Western ethnocentrism. For example, China was slow to adopt Western ways not because of backwardness but, on the contrary, because indigenous institutions worked so well that there seemed little need to change them. Such an insight—which Fairbank elaborates in this book with great force and clarity—is not easy for those who view the world in Europocentric terms.

There are other elements in Fairbank's approach, but by far the most important—his basic thesis for years—is the notion that understanding Chinese history is a prerequisite to understanding the modern Chinese revolution. Otherwise, as he says in the present volume, one is "committed to flying blind among the mountains" (p. 11).

One cannot deny that history illuminates the present, especially in China, where the Confucian-familial-agrarian past thrusts so insistently into the Communist present, and Fairbank's own work, including this volume, proves the point. Nonetheless, Fairbank's eagerness to promote that theme leads him, in this book as often in the past, casually to suggest misleadingly simple linkages between past and present. To take but three of many possible examples, Fairbank equates the Communist government's disinclination to develop a fleet, or to send expeditionary forces abroad, to decisions by Han rulers, some two thousand years ago, to limit expeditions abroad to a few weeks in duration (p. 358). He also suggests that "today's all providing, all controlling work unit [is] an updated form of the old family system" (p. 358). And he observes that Deng Xiaoping's post-Mao reforms "won public acceptance like those of the Han after the Heaven-storming tyranny of the Ch'in First Emperor [third century B.C.] or of the T'ang [seventh–tenth centuries after Christ] after the grandiose excesses of the Sui" (pp. 342–43). These easy parallels do not exemplify the kinds of continuity that dictate the need for historical understanding. They mislead and obscure more than they clarify, and they may subtly nurture the idea of an unchanging China, a cliché that Fairbank has always and rightly repudiated with vigor.

JAMES E. SHERIDAN
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MICHAEL A. BARNHART. *Japan Prepares for Total War: The Search for Economic Security, 1919–1941*. (Cornell Studies in Security Affairs.) Ithaca: Cornell University Press. 1987. Pp. 290. $29.95.

This book recounts the path to Pearl Harbor with fresh light on Japan's quest for economic self-sufficiency. Michael A. Barnhart has harvested Japanese army records especially thoroughly in compiling this chronicle of interservice squabbling in Tokyo and diplomatic misperceptions in Washington. Within the sharply defined focus he has chosen, the author provides useful new information about how the Japanese prepared for war.

Barnhart sketches the rise of Japan's "total war" military officers, who sought autarky for their country after seeing Germany's fate in World War I. Under army leadership their efforts merged with a bureaucratic drive for domestic reform, bringing civilian mobilization and national economic controls by 1937. Paradoxically, the war with China starting that year crippled the "total war" faction and elevated senior officers who favored war with the Soviet Union.

The author ably summarizes America's defense of Open Door principles in the 1930s, focusing on diplomats Joseph Grew and Stanley Hornbeck (who "was not the 'China crusader' he is sometimes made out to be" [p. 119]). Washington debated the wisdom of economic pressures on Japan in a fog of misinformation about its economy; eventually, the United States stopped sales of aircraft and equipment (September 1938), denounced the trade treaty with Japan (July 1939), and stopped exports of machine tools and high-octane aviation fuel (summer 1940).

As the war in China dragged on, the Japanese army, navy, and cabinet planning board bickered endlessly over allocations of strategic materials. The author argues that Japan's drive for autarky in continental Asia ironically increased its dependence on America—for scrap, copper, machine tools, and rarer metals, as well as oil. The Japanese navy, more and more strapped for steel, pressed for an advance into Southeast Asia lest it lose its allocation of materials to the northward-looking army.

The army first committed itself to a southward strike in July 1940, mainly for the resources of the Dutch East Indies; the navy's idea of a southward move was more gradualist but also more comprehensive, anticipating eventual war with the United States as well as Great Britain and the Netherlands. The American embargoes of mid-1940 (including scrap in September) hurt the Japanese economy badly. By the next April the army had emasculated the cabinet planning board, further starving the civilian sector but hardly denting the navy's competition for scarce commodities.

Barnhart highlights the U.S. freeze of Japanese assets in late July 1941. Before then the Americans had feared provoking Japan into further expansion, but now Washington thought expansion was inevitable and froze the assets to slow Japan's course. Henceforth the Japanese army suspended its plans to attack the Soviets. The author says the United States froze the assets without knowing of the deep rift between the army and navy in Japan (p. 227); he speculates that such knowledge might have let the Americans devise a strategy to exploit the interservice differences (pp. 264–65). He argues that a southward advance, now virtually inevitable, would drain the Japanese economy more immediately than the coming oil shortage (p. 249) and that steel shortages were as crucial as the oil crisis in the final debates of October 1941 (p. 255).

This book is strongest on Japanese military planning and weakest on Japanese government policy toward the civilian economy (business-military relations are barely mentioned). How and why the "total war" officers were shouldered aside is somewhat unclear in Barnhart's narrative, as is the importance of the Japanese-Soviet neutrality pact of April 1941. How Japanese planners assessed the European theater after Barbarossa in June 1941 is left in doubt, so too is the concept of American "appeasement" of Japan.

Most readers will welcome Barnhart's dissection of army-navy rivalries, although some may question his interpretation of the asset freeze. It is a pity that his publisher has produced this sound study in so slipshod a fashion—from proofreading to the runny red ink of the cloth bindings.

THOMAS R. H. HAVENS
Connecticut College

J. VAN GOOR, editor. *Trading Companies in Asia, 1600–1830*. (HES Studies in Colonial and Non-European History, number 3.) Utrecht, HES, 1986. Pp. 167. f. 29.50.

This rather loosely knit collection is especially notable for four essays on aspects of the European presence in Indonesia between about 1680 and 1830. These articles are especially challenging in the way they call into question our usual tendency to make the end of the Dutch East India Company in 1799 the great divide in this period and in the suggestions that the extension of European political power marked less of a radical break with the Indonesian political tradition than many scholars have thought. Thus, the volume will be important and stimulating reading not just for students of Indonesia but for all who are trying to understand the successive phases of Immanuel Wallerstein's "modern world system."

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The most provocative of the four essays on Indonesia is L. W. Nagtegaal's on relations between the local regents and the *susuhunan* or high king of Mataram in Java. Nagtegaal builds a careful analytic framework (which occasionally interferes with his sequential explanation of political change) of the nature of power within the regencies and of the regent-*susuhunan* relation, which he argues was frequently a coercive, not consensual, part of a political culture in which a ruler demonstrated his spiritual power largely by defeating his rivals. J. Van Goor's interesting picture of political and economic change in the Pontianak region on the west coast of Kalimantan presents a picture of a loosely structured and unstable political system much like Nagtegaal's picture of Java. In both of these essays good evidence is cited that the Dutch were considered normal players in local politics, to whom one leader or another was likely to turn in time of conflict. Most of this evidence is from Dutch sources, which makes especially welcome P. B. R. Carey's essay based on a Javanese manuscript recounting an aristocrat's experiences during the English occupation, 1811–16. There is much fresh insight here on the new political arrangements and cultural ambivalences of aristocrats who collaborated with the English and the Dutch. Nagtegaal and Van Goor do not neglect economic change and commercial connections with the Dutch, but G. J. Knaap's essay is a more distinctive contribution to this field. He describes clearly the amazingly rapid growth of coffee production in Java in the early eighteenth century but occasionally seems to understress the coercive and exploitative aspects of this change.

The other two essays in the volume have little relation to this Indonesian core apart from their involvement with Dutch sources and the history of the Dutch East India Company. One, by the distinguished Indian economic historian Om Prakash, takes up the very important question of the effects of silver inflow on the economy of Bengal in the late seventeenth and early eighteenth centuries, which he finds to have been generally positive. The very clear and systematic analysis is a little fuller than that in the last chapter of Prakash's *The Dutch East India Company and the Economy of Bengal, 1630–1720* (1985). The essay on the Dutch and the Japanese in the eighteenth century by M. E. van Opstall quotes some intriguing Dutch material, especially some glimpses of a Dutch conversation with a learned Japanese who probably was Arai Hakuseki, but has no very strong thesis and makes little use of Japanese sources and studies.

Van Goor's brief introductory essay focuses on issues in the chapters related to Indonesia, including those mentioned above. This modest paperback volume has been more carefully edited and proofread than some books being published by the most prestigious university presses. Some illustrations are of problematic accuracy, others not clearly related to the text. The book could have been improved by the inclusion of a glossary of Indonesian terms and by a mode of footnoting archival sources that specified the nature and date of the document as well as its location.

JOHN E. WILLS, JR.
University of Southern California

BRIAN E. MCKNIGHT, editor. *Law and the State in Traditional East Asia: Six Studies on the Sources of East Asian Law.* (Asian Studies at Hawaii, number 33.) Honolulu: University of Hawaii Press. 1987. Pp. ix, 182. $20.00.

Historians of premodern China, Japan, Korea, and Vietnam would do well to take note of the six diverse technical studies—first presented at a Harvard conference in 1978—that are gathered together here. Edward L. Farmer analyzes three early sets of the many regulations by which the founding emperor of China's Ming dynasty attempted social engineering on an ambitious scale. Ta Van Tai demonstrates that relatively egalitarian views of the civil rights of women in Vietnam were maintained in adaptations of the Ming code under the Lê dynasty and were perpetuated in practice thereafter. Carl Steenstrup reveals how litigations, especially about land law, were handled in the complicated court system of Kamakura Japan, which worked to the rulers' advantage by defusing potentially threatening political disputes. Brian E. McKnight examines Sung China's several categories of laws, which grew so complex that line officials could justifiably avoid hard legal decisions by passing cases to the throne for resolution, with the result that codified precedents became increasingly voluminous and rule became increasingly centralized and autocratic. Osamu Ōba discusses the circumstances under which, and the scholars by whom, Chinese codes and other legal writings were studied, translated, and interpreted in early Edo Japan. William Shaw explains some of the barriers, especially including entrenched Buddhist practices, that fifteenth-century Korean rulers had to overcome or accommodate in their fostering of neo-Confucian moral values and Ming laws. In a very brief introduction, McKnight points out that the diversity of the papers, in both subject matter and approach, reflects the still pioneering state of the comparative study of law in traditional East Asia.

CHARLES O. HUCKER,
EMERITUS
University of Michigan

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UNITED STATES

DAVID BRION DAVIS. *From Homicide to Slavery: Studies in American Culture*. New York: Oxford University Press. 1986. Pp. x, 305. $29.95.

Retrospective collections of essays by living authors seldom seem to work well. On rare occasions they jell famously, as with David A. Hollinger's *In the American Province* and C. Vann Woodward's *Point Counter Point*. Yet many such books—if one may judge from reviews in this and other journals—often sputter badly and essentially do not work.

David Brion Davis's present book does indeed work, but in an unusual way. Its nineteen essays range in time from 1954 to the 1980s. They reflect an intellectual odyssey that no one (including especially and assertedly the author) would have cared to predict some thirty-five years ago. They signpost an interesting, fruitful, and instructive journey, one that will interest readers of varying perspectives and interests.

The essays deal with such a disparity of subjects that it is hard to convey the subtle threads of their commonality. They have a penetrating and perceptive concern with the moral dimensions of American social identity, especially with violence and sense of masculinity. From various angles they view such facets of American culture as murder, capital punishment, Natty Bumpo, and the Marlboro Man. More general matters of social identity are raised in discussions of the American family and the Mormons. Somewhat more than one-third of the book deals with slavery and, especially, antislavery—themselves topics that bear on matters of identity, social consciousness, aggression, and violence. Chronologically, most of the book focuses on roughly the first half of the nineteenth century, although conceptually and materially the essays spill over both ends of the antebellum period.

The format of these essays follows an unusually regular pattern. With one exception they are either about ten or about twenty pages long. Many, though not all, of the ten-page pieces originally appeared in *The New York Review of Books*. These cover such variant matters as the westward movement in the United States, the Mormon church, Orlando Patterson's sweeping world-wide study of slavery, and the image of blacks in Western art. Among the twenty-page essays are several that have long since acquired solid reputations as important articles of the sort one inflicts on graduate students during one's better moods, for example, "Some Themes of Counter-Subversion: An Analysis of Anti-Masonic, Anti-Catholic, and Anti-Mormon Literature" and "The Emergence of Immediatism in British and American Anti-Slavery Thought."

It is not easy to discern what sort of essay still stands up best now and which will continue to in the future. Obviously, such a review article as "Slavery and the Post–World War II Historians" is partially self-dating, however useful and penetrating it once was. But, when "Some Themes of Counter-Subversion" appeared in 1960, who would have guessed that it would undergo less challenge than "The Emergence of Immediatism," which was published two years later? And, more strikingly, who in 1957 would have predicted that an article entitled "The Movement to Abolish Capital Punishment in America, 1787–1861" would have taken on poignant and powerful immediacy thirty years later?

The only essay that exceeds the ten- and twenty-page pattern describes James Cropper's role in the British antislavery movement during the 1820s. Granting Cropper's importance, I think that his convolutions have here been treated with greater attention to detail than is likely to interest most readers of this book.

With this collection one especially appreciates lucid analysis on such a wide variety of topics. Because readers' tastes vary so much, it ought to be reported that, except for a brief introduction, the book does not offer running and "updating" commentary about each essay. Although such reflections are sometimes fun and interesting, they can get onto pretty sticky ground very fast. In this case I think the author did well to let the essays stand on their own independent hind feet.

WINTHROP D. JORDAN
University of Mississippi

MARTIN E. MARTY. *Religion and the Republic: The American Circumstance*. Boston: Beacon. 1987. Pp. viii, 391. $25.00.

Martin E. Marty is one of the most prolific writers in the historical profession today. During his distinguished career, he has produced, on average, almost one book per year. His *The Infidel* (1961), *Righteous Empire* (1970), *Pilgrims in Their Own Land* (1984), and *The Irony of It All* (1986) have all been major contributions to the field of American religious history. In *Religion and the Republic*, Beacon Press has collected a series of Marty's essays, written over a seventeen-year period, and made them available in book form.

Ordinarily, such compilations have a cobbled together feel. Marty attempts to avoid this by placing the articles within an overall framework of "homogeneity versus religious pluralism." The essays contain occasional repetitions of both phrase and quotation, and it takes some tugging to make them all fit. But, in the main, the framework holds. This book emerges as a paean of praise to Father John

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Courtney Murray's observation that "religious pluralism is integral to the human condition."

Marty's essays touch on a variety of themes: republicanism, civil religion, Reinhold Niebuhr as "the classic public theologian," spirituality in a secular world, the Bible as an icon in the American republic, foreign visitors' perceptions of the nation's religious life, the collapse of the Catholic "ghetto," southern evangelicalism, fundamentalism, and the Mormons.

The author strongly denies that there has ever been a time of religious homogeneity in American history. But he notes that ethnic-religious tensions have frequently been directed against factions *within* the group, rather than against the "dominant" (for many years, Protestant) culture. In other essays Marty discusses the emergence of a "Protestant ghetto" and wrestles with the political implications of the recent conservative evangelical upsurge. His analysis of the dilemmas of a "gentile" historian confronting the birth of the Mormon faith is especially well drawn.

Marty's ear is largely tuned to the dilemmas of contemporary religious life. He looks to the past for examples to illuminate these problems. If there is occasionally more soufflé than main course here, the essays still have a great deal to say about religion in American life.

FERENC M. SZASZ
University of New Mexico

JAMES R. MASTERSON, compiler. *Writings on American History, 1962–1973: A Subject Bibliography of Books and Monographs*. In ten volumes. White Plains, N.Y.: Kraus. 1985. $1300.00 the set.

Since 1902 the series *Writings on American History* has been the comprehensive bibliography for scholars of the nation's past, but recently the means of publication fell behind annual scholarly production itself. Books and articles produced in 1961 were compiled in a two-volume bibliography that was not published until 1978. Articles alone have been published in subject bibliographies through 1985. The book and monograph bibliography for 1962–73 has now been released in ten volumes, still more than a decade in arrears. More than fifty thousand works are listed, some in more than one category, resulting in roughly eighty-eight thousand entries with full bibliographic information, including Library of Congress catalogue number and, when assigned, the Dewey Decimal classification number. Especially informative is the listing of the table of contents of books that are collections of essays.

The major difference between the "old series" of *Writings on American History*, begun by the AHA, covering the years through 1960, and the "new series" jointly published by the AHA and the Kraus-Thomson Organization, is that annotations have been dropped in favor of categorizing entries by subject. The volume of scholarship has become too large to finance annotation.

The bibliography is divided into five categories, within which subdivisions cover more specific topics. "History and Historians," in 250 pages, includes historiography broadly defined, libraries, and oral history. "Chronological Classification" covers 400 pages of titles, not all of which are listed elsewhere by specific subject. "Subject Classification" occupies more than half of the ten volumes, covering cultural and intellectual history (over 600 pages), the history of science and technology (over 100 pages), economic history (400 pages), communications history (less than 100 pages), transportation history (100 pages), military history (250 pages), political history (almost 700 pages), social history (1300 pages), and exploration and discovery (150 pages).

Some summary observations can be made. Titles categorized as "social history" and "cultural and intellectual history" together exceed all the others combined. Assuming consistency in the categories over the years, this represents an obvious flowering during 1962–73 of these relatively late-developing fields of historical study. Because the entries are not organized chronologically within the subject categories, one cannot easily assess whether cultural and intellectual history appeared more popular earlier or later during 1962–73, in relation to social history.

Students of particular specialties may derive suggestions of historiographical trends by comparing subject allocations during 1962–73 with earlier periods. For example, from the entries in volumes of *Writings* between 1918 and 1954 an average of approximately 15 percent fell into categories now considered to be cultural and intellectual history. The same categories comprise roughly 20 percent during the 1962–73 period. It is not surprising that an increase in cultural and intellectual history titles should be listed in the more recent years, but it is surprising that there was not much change between World War I and the 1950s. The value of such bibliographic subject comparisons may be reduced, of course, by arbitrary and inconsistent categorization over the years.

"Geographical Classification," covering 1100 pages, devotes roughly 200 pages each to the South and the Middle Atlantic, 150 pages to the Pacific (Hawaii, Alaska, Pacific Northwest) and the Middle West–Old Northwest, 100 pages each to New England and the Southwest and less to the Border states (Arkansas to West Virginia), the Rocky Mountains, the Great Plains, and territories, possessions, and dependencies. The final section of 500 pages, "Biography," is mainly devoted to individual biographies. Volume 10 contains an index of authors,

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editors, and compilers and an index of personal names about whom books were written.

Despite the movement toward social and intellectual history during 1962–73, political figures continued to dominate biographical writing. Abraham Lincoln far outnumbered all other Americans featured in biographical studies with 99 titles listed, followed by John Kennedy (59), George Washington (47), Thomas Jefferson (39), Benjamin Franklin (37), Robert Kennedy (34), Franklin D. Roosevelt (31), Martin Luther King (29), Lyndon Johnson (25), Richard Nixon (23), and Woodrow Wilson, Dwight David Eisenhower, John Dewey, and Ernest Hemingway with 20. Eleanor Roosevelt, with 13 entries, is listed more often than any other woman.

This impressive and useful bibliography is technically well executed, although readers will discover occasional proofreading mistakes. The most frequent is a failure to consolidate authors' names picked up by the computer in different forms. For example, John Higham and J. Higham are the same person, and the different index references to the works of each should be consolidated. There are approximately 200 such instances of multiple listings for names beginning with *H* alone. In two cases of *H* names, the authors are listed four different ways, each with references to different works.

Readers will turn the pages of the bibliography with an appreciation for the glacial character of historical scholarship. The peaks and valleys of revisionist interpretation by the most visible historians are flattened out by the thousands of scholars whose smaller contributions move the field monograph by monograph. The years 1962–73 saw dramatic interpretive conflicts. "Consensus" interpretations opened the period, and "New Left" ones closed it. Ethnic and women's history studies stimulated social history. The historical profession went from the height of self-confidence to the depths of self-examination, inquiring as to possible meanings to be derived from historical understanding or relevance to contemporary problems. Undergraduate history course enrollments declined at most institutions during these years, and historians by the end found themselves in an employment depression. These developments can barely be discerned in the massive bibliographic catalogue, which nevertheless remains the invaluable foundation for our knowledge of historical scholarship and an equally invaluable tool for scholars. The AHA is to be congratulated for keeping the project alive.

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ROGER L. NICHOLS, editor. *American Frontier and Western Issues: A Historiographical Review.* (Contributions in American History, number 118.) New York: Greenwood. 1986. Pp. viii, 303. $35.00.

This volume, with its awkward but descriptive title, will inevitably be compared with *Historians and the American West* (1983), another collection of historiographic essays, edited by Michael P. Malone. Indeed, many of the subjects of those chapters are covered again here: continental expansion, territorial government, the frontier army, environment and ecology, economic development, farming, ranching, and mining, transportation, urbanization, Indians, ethnic groups, and women. Frontier social history is a new topic here, but the historiography of the fur trade, the Southwest, Mormons, violence, and frontier culture are largely ignored. Happily, however, this collection adopts a fruitfully different focus and approach. First, as Rodman W. Paul noted in his foreword to Malone's collection, that survey concentrated on the standard works of the field and gave the impression that practitioners of western American history, in Paul's words, "have as yet ventured only cautiously and partially into the ferment of new thinking that has characterized the profession nationally." Noting that Malone's contributors represented a group of older western historians, Paul wondered what "the oncoming generation" would emphasize in a similar review. The authors of the present essays represent a range of ages, but as a group they are notably younger, and their historiography is notably influenced by the new methodologies and subject areas that have characterized the development of history in the past quarter-century. This feature alone marks this volume as an important addition to the historiographic discussion; here is the first place to look for bibliography and review of some of the freshest and most provocative historical work in the field of western history.

Moreover, whereas Malone's volume was largely dedicated to the historiographic discussion of the West as region, editor Roger L. Nichols has assigned his contributors the task of considering their subjects in relation to issues of settlement and development, that is, to the frontier as process as well as to the West as place. Admittedly, some of these essays remain bound in a regional strait jacket, but others range widely and imaginatively over the centuries of American history. Unfortunately, since as yet few have been able to agree on what is meant by the term "frontier," this focus also produces a good deal of conceptual confusion. While Nichols candidly admits to this difficulty in his introduction, the contributors nevertheless might have reached for an intellectual consensus—at least some evidence of struggle would be heartening. Over the past decade a few pioneers have proposed bold and theoretically sophisticated approaches to frontier history—

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Jerome O. Steffen's *Comparative Frontiers: A Proposal for Studying the American West* (1980) and Howard Lamar and Leonard Thompson's *The Frontier in History: North America and Southern Africa Compared* (1981) leap to mind. It is frustrating to see these efforts treated as simply more material for the bibliographic potpourri rather than as theoretical guideposts for structuring the critical discussion.

This collection and Malone's are a part of a wider self-examination taking place among practitioners of western American and frontier history. Judging from the comments in these various critiques, one might conclude that this cohort of historians suffers from an inferiority complex. There is a horror of falling into "gunsmoke and horse manure" history, on the one hand, and an almost hagiographic respect for the great historians of the past such as Frederick Jackson Turner and Walter Prescott Webb, on the other. This is very curious. Like their southern counterparts, many historians of the American West traditionally felt isolated from eastern intellectual institutions and were correspondingly defensive and clannish about their own field. Unlike southern history, however, western history lacked an established place in the fundamental periodization and signification of the American past. It is precisely because Turner and Webb offered interpretations of frontier and West that placed the subject squarely within the mainstream of American history and culture that they continue to loom over western historiography long after their deaths.

To be sure, as these essays point out, there is yet much to be learned from rethinking the work of the masters and even more from a reconsideration of less well known progenitors such as Herbert Bolton, Carl Sauer, Sherburne Cook, and James C. Malin. But this survey provides abundant evidence that the western and frontier history being written today obviates the necessity of knee-jerk ancestral obeisance. Although some of the subject areas covered here seem little charged by recent historiographic developments, others are surging with energy. Worthy of particular note are work in geographic and ecological history (including, among others, the work of D. W. Meinig, William Cronon, Alfred Crosby), in the ethnohistory of Indians (Richard White, R. David Edmunds, Calvin Martin, Rayna Green), in the black experience (Rudolph M. Lapp, Nell I. Painter, Robert G. Athearn, Lawrence B. De Graaf), in the Mexican experience (Mario T. Garcia, Arnoldo De Leon, Ricardo Romo, Darlis Miller), in the study of western communities (Kathleen Neils Conzen, Malcolm J. Rohrbough, Mark Wyman, Ralph Mann, Frederick Luebke), and in the history of women (Glenda Riley, Sandra Myres, Sylvia Van Kirk, Susan Armitage, Anne Butler). Historians are well on the way to imagining a complex, multicultural West, linked to the expansion of capitalist Europe, akin to the worldwide history of colonialism and imperialism.

Although many, perhaps most, members of this "oncoming generation" work and teach in the West, many do not, and one suspects that this process of specialization and nationalization, which is at once both opening up and threatening the coherence of the field, is very much a part of the self-perceived "crisis" of western history that produces volumes such as this. As one contributor notes, much of what might be considered "frontier" history is claimed by the energetic legions of "colonial" history; often, good journal articles go to the *William and Mary Quarterly* rather than to the *Western Historical Quarterly*. But one hopes that frontier and western historians will realize that the tradition of popular fascination with the "Wild West" is less opprobrium than opportunity, not a problem but a "problematique" that should infuse our work. We ought to avoid the tendency evident in these essays to throw the baby out with the bath water; as Richard Maxwell Brown suggests, there is great prospect in linking the "Wild West" with the "workaday West" in the same histories; there is no reason to abandon the special qualities of the field that are themselves a product of America's history. Finally, what is most important is that western and frontier historians produce well-researched, theoretically informed, and provocative work that adds to the continuing discussion and debate about our past. This review suggests that western history has never been in better shape.

JOHN MACK FARAGHER
Mount Holyoke College

JOHN V. ORTH. *The Judicial Power of the United States: The Eleventh Amendment in American History*. New York: Oxford University Press. 1987. Pp. ix, 231. $24.95.

In the preface to this book on the Eleventh Amendment, John V. Orth says that he hopes "to explain one of the most obscure parts of the Constitution and to do it in terms understandable to the educated public" (p. vii). He is, on the whole, quite successful. As he rightly notes, it is an unusual feature of a book of legal scholarship that it employs a writing style that makes it easily accessible to the nonscholar, and Orth must be commended for doing this well.

The book is much more a general history of the Eleventh Amendment than an essay in legal analysis per se. Early chapters recount the background and early history of the amendment: the case of *Chisholm v. Georgia*, which upheld the suability of states by citizens of other states in federal courts, the rapid passage of the Eleventh Amendment to protect the states' ability to refuse to pay their debts without

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being haled into federal courts by their creditors, and attempts by early justices (such as Chief Justice John Marshall in *Bank of the U.S. v. Osborn*) to limit the expansive implications of some readings of the amendment, which would have been a substantial bar to review of many state acts.

The middle chapters—the "heart" of the book analytically as well as figuratively—describe how judicial interpretation of the amendment was conditioned by the events of Reconstruction. The immediate postwar "reconstructed" governments ran up huge debts, but later governments elected after the Compromise of 1877 (which marked the withdrawal of the North from internal southern affairs) repudiated them. Because the practical reality was that the Court would not be able to enforce judgments against southern states, it generally adopted broader views of the Eleventh Amendment, that is, ones that insulated the states from most such suits. In this process the amendment was reconceptualized in the broader terms of the doctrine of sovereign immunity.

The last chapters survey later efforts of the Court to interpret the amendment. Although sovereign immunity and the Eleventh Amendment have generally fared well, the demands of national supremacy and the rule of law have provided incentives to limit them as well, and so the court has followed a rather shifting path. The landmark case of *Ex parte Young* effectively resurrected *Osborn* and made it possible to sue state officials who acted unconstitutionally. And, through the laissez-faire era and into the modern era of civil liberties, the Court has discovered other exceptions and then limits to the exceptions. The result of this push and pull between sovereign immunity and contrary demands "has naturally been distorted doctrine, what the latest scholars call 'the theoretical incoherence of eleventh amendment jurisprudence'" (p. 152).

Orth nicely lays out this history in simplified form, providing an extensive bibliographic essay at the book's end for readers who want a more detailed treatment. Also included is a brief epilogue that focuses on the relation between legal analysis and legal history. Some readers—especially those less oriented toward legal realism—may find a limitation in the book's particular historical focus. For example, those who take their orientation in constitutional analysis from "original intent" will appreciate the explanations of how the amendment has been diverted from that intent in the course of history, but they will be bothered by the absence of sharper (more precise and more sustained) legal analysis—an absence that reflects Orth's willingness to let history rather than logic (that of the Constitution, understood in the light of the political philosophy that informs it) be the dominant prism through which constitutional law is viewed.

But readers will generally agree that this book is a very good introduction to the subject, and many of them will be very grateful for being able to obtain it in an unusually literate form.

CHRISTOPHER WOLFE
Marquette University

VICTOR R. GREENE. *American Immigrant Leaders, 1800–1910: Marginality and Identity*. Baltimore: Johns Hopkins University Press. 1987. Pp. xii, 181. $22.50.

That individuals do matter is the highly suggestive thesis of Victor R. Greene's slim volume on European immigrant leadership from 1800 to 1910. In 121 pages he covers the Irish, Germans, Norwegians, Swedes, Jews, Poles, and Italians. An additional 21 pages are taken up by an excellent summary of major theories on assimilation by anthropologists, sociologists, and historians, including his own.

He particularly takes issue with quantitative historians, writing social history "from the bottom up," who see the immigrants as "totally proletarian [and] virtually leaderless" (p. 4). In regard to sociologists and anthropologists who recognize the leader's role as broker-mediator but interpret the position between two or more partially antagonistic cultures as one of painful marginality, Greene suggests that immigrant leaders were more likely to be the "founding architects of organized group institutions" (p. 15) in which immigrants could "feel secure about the compatibility of their ancestral heritage and the principles of their adopted country" (p. 16).

For the Irish as well as the Poles, this compatibility took the rationale that to participate in the American political process was to set an example for their conationals across the sea for establishing successful republics of their own. For the Jews, to support "American humanistic, democratic principles" was to foster an environment particularly favorable to their own Yiddish folk culture. Similar intellectual resolutions to "the problem of being ethnic in America" were devised by leaders of the other groups. (p. 16).

Aside from the Scandinavians, each group, with individual biographies of its leaders, is given a short chapter of its own. Significant features common to the leadership of different ethnic origin thus tend to fall between the cracks. Just one case in point is that one-fourth were clerics or started out as such. No real comparative analysis is made of the pivotal role played by the Catholic church and its spiritual and lay leaders in the assimilation of Poles and Irish. Only for the Italians, his best chapter, does he demonstrate how the "Italian peasant's need for personal protection" (p. 124), together with his

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arrival here as an illiterate, single male, led to the rise of the padrone-banker type of leader.

Other distinct leadership types do not emerge, possibly because Greene's focus on the individual virtually precludes analysis of ethnic and socioeconomic factors conducive to leadership being assumed by some kinds of personalities and not by others. Yet the biographical approach, with its colorful detail, makes his synthesis well worth reading.

G. A. DOBBERT
Youngstown State University

MARK ERLICH. *With Our Hands: The Story of Carpenters in Massachusetts*. Assisted by DAVID GOLDBERG. Philadelphia: Temple University Press. 1986. Pp. xiv, 239. $29.95.

Mark Erlich adds something of value to the existing work on the carpenters' union by Walter Galenson and Robert Christie as he moves beyond the union to explore the work culture of the rank-and-file carpenters. Although the book focuses on Massachusetts, much of his material is generally applicable.

Erlich demonstrates how this work culture grew from the particular economic conditions in the trade. Most central was the lack of job security in a trade of scattered, finite projects that responded closely to each turn in the economic cycle. The insecurity of the work led to unions that stressed the closed shop, restrictive entry, and aggressive jurisdictional claims. Carpenters had a clear sense of self-identity and individualism based on their skill. This was, however, in uneasy tension with a cooperative and egalitarian relationship with fellow carpenters as reflected in union work rules and the standard daily wage. Thus, the carpenters exhibited solidarity within their craft and its union but indifference or hostility to outsiders, including other workers. The carpenters had craft consciousness not class consciousness.

A major section of the book concerns events since World War II. Erlich clearly shows why union construction has declined significantly nationwide since 1970. He argues for his union (Erlich is a member of Carpenters' Local 40) to become more democratic and open and to organize more widely, build stronger political alliances, and stoutly defend union standards. However, he retains enough respect for his fellow unionists who may disagree that these chapters remain good history, not disguised advocacy.

I question Erlich's conclusions on some specific points. These include a weak treatment of the eight-hour movement of the 1880s and 1890s and insufficient evidence to support either supposed political activity by carpenters or a significant level of sympathy strikes in the early twentieth century. However, these do not diminish the basic value of the book.

Erlich does not pretend to be dispassionate, but he does meet his own test of fairness. He refuses either to attack the carpenters or to ignore their easily discovered blemishes. Instead, both are part of a work culture that reflects the specific conditions of their trade. Erlich notes the uneasy balances that often resulted, and he tries not to oversimplify. Generally, he succeeds in this difficult task. In addition, the book points to the importance of studying workers within their unions. This is a message that should be noted by all labor historians.

IRWIN YELLOWITZ
City College
City University of New York

JOHN S. GILKESON, JR. *Middle-Class Providence, 1820–1940*. Princeton: Princeton University Press. 1986. Pp. 380. $29.95.

John S. Gilkeson, Jr.'s, book traces the history of middle-class voluntary associations in Providence, Rhode Island, from the antebellum temperance and antislavery movements through the consumer movement of the 1920s and 1930s. It begins by examining the origins of middle-class consciousness in the efforts of antebellum reformers to set apart the stable, industrious, and sober "producers" of society from the vices of the proslavery rich on the one hand and of the intemperate poor on the other. It then traces the institutionalization of those middle-class values in the didactically righteous civic culture centered on lyceums, libraries, and museums that dominated Providence between 1850 and 1880. But the broad producers' community gradually fragmented in the second half of the century, and the once inclusive civic culture shattered into hundreds of clubs and lodges organized around differences of class, sex, age, ethnicity, and neighborhood. After 1880 middle-class responses to this social and cultural fragmentation took four different forms: the campaigns of Single Taxers, Christian socialists, prohibitionists, and others who championed political nonpartisanship "in a vain attempt to re-create the antebellum moral community" (p. 300); the efforts of settlement and recreational reformers to reconstruct urban society around settlement houses and other neighborhood centers; the movement by businessmen, social workers, architects, and city planners to reintegrate their society along functional lines through federation; and the organization of housewives, academics, and white-collar workers as consumers. Although all these efforts ultimately failed to restore Providence to social and cultural homogeneity, Gilkeson argues, it was through such cultural imperialism that mid-

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dle-class Americans continued to define themselves as a class down to World War II.

Lucidly written, commendably researched, the book does a fine job of documenting the segmentation of civic life in Providence and demonstrating repeatedly what Gilkeson calls "the irony of middle-class organization" (p. 347): after 1880 the proliferation of new voluntary associations only called into question the continuing claim of middle-class Americans that their class interests were identical with the public interest. At times the work's declension framework hinders the sharpness of its analysis, overstating the homogeneity of antebellum Providence and the civic culture of midcentury, assuming too readily that the associational efforts of late nineteenth-century reformers represented a nostalgic attempt to restore that earlier "producers' community," and generally seeming to presuppose the superiority of the antebellum middle-class consensus to late nineteenth-century pluralism. It is somewhat disappointing too that what begins as a study in class formation comparable in significance to Mary P. Ryan's *Cradle of the Middle Class* (1983) and Paul E. Johnson's *A Shopkeeper's Millennium* (1978) evolves into a discussion that treats its central subject—voluntary associations—as reflections of other social forces such as urban growth, the "new" immigration, and the shift from a production to a service economy. Along the way, the narrative sometimes gets bogged down in the exhaustiveness of Gilkeson's research; chapter 4 in particular is essentially a catalogue of clubs and lodges. But, these criticisms notwithstanding, the book offers a useful community study that traces some important shifts in middle-class associational activity over more than a century and in so doing touches on dozens of issues—the class functions of antislavery, the evolution of civic institutions, the commercialization of leisure, the professionalization of housework, the emergence of urban planning, to name just a few—that will be of interest to a wide range of scholars.

KAREN HALTTUNEN
Northwestern University

JOHN E. FINDLING. *Close Neighbors, Distant Friends: United States–Central American Relations.* (Contributions in American History, number 122.) New York: Greenwood. 1987. Pp. xvi, 240. $35.00.

Of the seven hundred or so books (most of them by small presses) published on Central America in the past decade, surprisingly few attempt to synthesize U.S. diplomatic relations with the isthmus. Walter LaFeber's *Inevitable Revolutions* (1983) exploited largely economic themes in the century from the 1880s to portray modern Central America's explosive political and social climate as the inevitable product of isthmian dependency on the United States. In this briefer account, John E. Findling narrates the diplomatic relationship from the 1820s and the first expressions of American interest in Central America as a passageway between the oceans to the potential quagmire it has become for Ronald Reagan. He tries but lamentably fails to capture LaFeber's outrage over the sordid record of American policy and the sharp focus that author brought to his account of American failure in the isthmus.

The result is a book that demands at least twice as much space as Findling devotes to the subject. Two chapters survey the entire nineteenth century, admittedly an era of lesser interest to modern American readers of the historical record of U.S. relations with Central America. With the twentieth-century account, Findling provides more substance and, despite the brevity of the book, manages to devote an entire chapter to the Reagan "watch" over the isthmus. All too often he jumps from one country to another in a single chapter (a leapfrogging difficult to avoid when one is narrating the history of American relations with Central America) and in the process frustrates a reader looking for a central theme. He is critical of American policy though not as condemnatory as LaFeber. U.S. failure in Central America, he argues, may be largely explained by Americans' refusal to reflect on their historical record in treating with the region. But neither Findling nor most other American critics of U.S. policy in the region attribute Central America's troubles as much to choices Central Americans have made as to the relentless political and economic intrusion of the United States. As have others, he attributes Reagan's alarmism over Central America to the president's belief that America's global prestige is at stake there rather than to persuasive evidence of American strategic interests.

In providing a brief summary of American relations with Central America and in identifying the relevant American sources and scholarship, Findling has succeeded. But he has not really explained U.S. policy nor analyzed the sometimes powerful private interests that have occasionally influenced it. And he knows too little of Central America to appreciate the barriers that small, weak countries have been able to erect against American intrusion and dismisses too quickly their ability to manipulate the more powerful outsider whether it comes to do them ill or good.

LESTER D. LANGLEY
University of Georgia

HILLEL SCHWARTZ. *Never Satisfied: A Cultural History of Diets, Fantasies, and Fat.* New York: Free Press of Macmillan. 1986. Pp. vii, 468. $19.95.

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In this engaging, witty, sprawling, provocative book, Hillel Schwartz tells us a great deal indeed about the cultural meanings both of fat and of the efforts to get rid of it. So replete with eclectic information and clever asides is the book that it threatens at times to become more encyclopedia than analysis. At best, however, the author's self-awareness gives the book a sharply satirical quality.

With more than passing reference to European history since the eighteenth century, Schwartz concentrates on American culture since the Jacksonian period. He discusses in great detail a panoply of historical definitions of the physical self, nutritional theories, and dietary fads. He demonstrates convincingly that this constellation of concerns has amounted to an unending and profound obsession for perhaps the majority of Americans and that this obsession has been capitalized on by an army of cranks and confidence men. So desperate to diet have many Americans been that, abetted by their doctors, they have resorted to many downright dangerous practices, such as the amphetamine craze of the 1950s.

On broader levels of analysis, the book is less satisfying than on particulars. This is in part because of Schwartz's tendency to pick up a thoughtful theory only to abandon it after a few pages for another, often equally interesting, possibility. Diet as a form of evangelical conversion and thinness as an appropriately self-disciplined Republican body type or as romantic buoyancy, to take three of his examples, are all quite suggestive theories, yet none are incorporated into a larger and convincing general analysis.

What does unify the book is an underlying antipathy to a culture dedicated to thinness and dieting. In his final, humorously enraged chapter, "Fat and Happy?" Schwartz finally makes his position explicit. The cultural presupposition about fat people in a success-worshiping nation is that "because they have failed they are fat, and because they are fat they fail" (p. 321). Like other minorities to whom negative symbols are attached, fat people are "compromised and persecuted" for their weight (p. 322). As an antidote to this cultural bullying of the fat, Schwartz imagines a utopia in which Americans would not deprive themselves and hence would not need to dominate others. In a New Left manner, Schwartz sees dieting as a badge of narcissism, an emblem of the ruthless impact of capitalism on individual character. People ought instead just to relax a bit, rest more comfortable in their own bodies, and become more accepting of a whole range of body shapes. Amen to that.

MICHAEL FELLMAN
Simon Fraser University

TERESA TOULOUSE. *The Art of Prophesying: New England Sermons and the Shaping of Belief*. Athens: University of Georgia Press. 1987. Pp. xii, 211. $23.00.

The study of early American religion has been enriched in recent years by the efforts of scholars such as Charles Cohen and Ann Kibbey to go beyond the surface of faith and examine the emotional and symbolic meanings of religious formulations. The inspiration for many such studies has come from the study of literature, and that is certainly the case with this book by Teresa Toulouse. *The Art of Prophesying* is an examination of the preaching of John Cotton, Benjamin Colman, William Ellery Channing, and Ralph Waldo Emerson, with attention to how form was influenced by and in turn influenced the message of faith.

The design of the study limits the degree to which it can illuminate the general subject of New England sermons and preaching. William Perkins's *The Arte of Prophesying* (1592) is used as a norm against which later developments in sermon form are measured, but there is no effort to demonstrate how widely accepted Perkins's advice was. Subsequent chapters trace divergence by each of Toulouse's subjects from the practice of his predecessor, but none of these men was typical of the clergy of his age. The author herself describes Cotton's preaching as "unique and problematic" (p. 29) and Channing as "something of an anomaly" (p. 79), and her examination of Colman and Emerson clearly demonstrates their positions outside the mainstream of clerical belief and practice. Discussion of Harry S. Stout's findings (*The New England Soul: Preaching and Religious Culture in Colonial New England* [1986]), based on the reading of over two thousand colonial sermons, might have helped establish a context for these sketches. Stout's volume was probably too recent for Toulouse to have had the opportunity to reflect on it, but her failure to cite earlier key studies of New England theology (for instance, Dewey Wallace's *Puritans and Predestination* [1982]) is less explicable and indicative of some of the perils of cross-disciplinary studies.

Despite some shortcomings, this is a book that all students of early American religion can profit from. Toulouse succeeds in showing how a preacher's message and his presentation of that message were influenced by a sense of his audience. She develops many valuable insights, as in her discussion of the parallels between Colman's use of emotional language and that of the English Dissenting clergyman Isaac Watts and poet Elizabeth Rowe. Her textual analysis of the preachers' sermons enables her to make useful observations on where each clergyman identified the source of religious truth. The four figures studied may not have been representative, but they were important, and Toulouse succeeds in

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bringing us to a better understanding of their religious views.

FRANCIS J. BREMER
Millersville University of Pennsylvania

PATRICIA U. BONOMI. *Under the Cope of Heaven: Religion, Society, and Politics in Colonial America.* New York: Oxford University Press. 1986. Pp. xii, 291. $24.95.

Many of us in American religious history cut our teeth on tales of the "declension" of *fin-de-siècle* eighteenth-century churches, with estimates that, say, only 10 percent of the colonial population were churched. Now Patricia U. Bonomi has changed that. Building on her pathbreaking article with Peter R. Eisenstadt, "Church Adherence in the Eighteenth-Century British American Colonies" (*William and Mary Quarterly* [1982]), and on the work of other revisionist scholars such as Richard Pointer (*Protestant Pluralism and the New York Experience* [1988]), Bonomi argues a convincing case for the vitality of eighteenth-century religion. With its broad yet focused survey of the entire Atlantic seaboard, her sketch of colonial religious life presents us with churchfolk who, outside of New England at least, thrived on pluralism and diversity. Armed with new estimates that "no less than some 60 percent of the adult white population attended church regularly between 1700 and 1776," Bonomi concludes that "the notion of an eighteenth-century-long hiatus, or trough, in American religious observance has little historical logic" (p. 220).

But it is the relation of this energetic colonial religion to the political life of the times that perhaps most interests Bonomi. In an exploration that in terms of theme and conclusion evokes a much controverted thesis, she argues persuasively for the links between the Great Awakening and the revolution. Bonomi, however, parts company with Alan Heimert and those who have followed him in her refusal to mount a case in terms of intellectual history. "Should we move beyond theological differences . . . to consider the revival's impress on patterns of leadership and on popular participation in organized opposition to authority," she advises, "we might discover a more pivotal linkage between the Awakening and the Revolution" (p. 161). And Bonomi does so with intelligence and sensitivity, working as a social historian to see how the organizing impetus of the awakening and its rhetoric of opposition molded Americans for the next major (political) step in their history.

If her study of the connection between awakening and revolution may be faulted in its interpretive reading, it is because Bonomi does not add the psychosocial to the generally social, as the work of scholars such as Jay Fliegelman (*Prodigals and Pilgrims* [1982]) has taught us to do. And, if the larger portrait of colonial religious vigor may be faulted, it is because Bonomi does not push sufficiently to discover the nature and intensity of internal piety in the midst of seemingly avid external practice. No doubt, though, this is to ask Bonomi to write a study she did not write. Now that, thanks to her efforts, we know about the flourishing state of eighteenth-century religious practice among the colonists, we are in a better position to ask other questions about it.

CATHERINE L. ALBANESE
University of California, Santa Barbara

ALAN VANCE BRICELAND. *Westward from Virginia: The Exploration of the Virginia-Carolina Frontier, 1650–1710.* Charlottesville: University Press of Virginia. 1987. Pp. ix, 227. $22.50.

Alan Vance Briceland hypothesizes that historians of the seventeenth century have either misunderstood or misinterpreted the relationship between Virginians and the frontier. Specifically, he examines four recorded journeys of exploration between 1650 and 1710. By comparing the landmarks noted in these travel accounts with the information available from modern U.S. Geological Survey 7.5 degree quad maps, the author has reconstructed what he believes is the correct route followed by the travelers. Two important reinterpretations of early American history will be necessary for those who accept Briceland's postulates: that Edward Bland's pioneering journey was to the south, not to the west as has long been believed, and that some of these early explorers did in fact cross the Appalachian barrier and find their way to the headwaters of a westward flowing stream. Frontier studies that follow this new interpretation, moreover, must now describe the seventeenth-century Indian trade in a slightly different way, since the author argues that exploration, not economics, prompted these journeys. If Briceland is correct, serious revisions must be made in our descriptions of the seventeenth-century Anglo-Indian frontier.

Two limitations about the study should be noted. First, the argument is forwarded that exploration of the early Virginia frontier was hindered by fear. The forest, writes Briceland, was a place filled with dread. For many readers there will be an apparent contradiction; it is hard to accept the notion that souls brave enough to sail across the bosom of the deep would shrink in terror from the forest. Although the author's interpretation may be accurate, the documentation and argumentation on this point are inadequate. Some reference to either literary

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allusions about or scholarly explanations of this fear would certainly strengthen the point. Second, there is the problem of Briceland's understanding of the seventeenth-century Indian. Neither the author's explanations nor his notes reflect familiarity with the growing ethnohistorical literature on the subject.

Despite these reservations, this is a thoughtful, provocative volume. The author's keen eye for detail has led him along a number of trails, some false, some true, which he has now cleared for the rest of us to follow. He is also to be congratulated for combining historical and geographical data to use as a tool for opening a path long hidden. Students of seventeenth-century Virginia and the southeastern frontier are indebted to this work.

JAMES H. O'DONNELL III
Marietta College

EDWARD BYERS. *The Nation of Nantucket: Society and Politics in an Early American Commercial Center, 1660–1820.* (New England Studies.) Boston: Northeastern University Press. 1987. Pp. xvi, 367. $35.00.

Dominated by Quakers, tolerant of dissenting religions, reluctant to use coercive power, commercial at its inception, possessed of a large native population, Nantucket seems to be the antithesis of the typical New England town. One can search Nantucket's records in vain to discover a Puritan village such as Sudbury, Dedham, or Andover. Edward Byers's book, however, questions whether the Puritan village deserves its reputation as the typical New England town or whether there ever was a typical New England town. Nantucket does not conform to the image of a village of farmers grouped around a Congregational church, but neither do the majority of colonial towns examined in the most recent wave of local history. Byers states explicitly what has become increasingly obvious to many historians in the past decade: "A new generation of community studies shows a degree of market orientation, individualistic behavior, contentiousness and social disorder, wealth stratification, and economic exploitation that would surprise readers who have relied on earlier historians for their picture of New England village life" (p. 10). These capitalistic, commercial towns are one of several types of communities that filled out the colonial New England landscape.

Although Byers is by no means one of the pathbreakers in this attempt to redefine the New England town, his book is almost certain to become one of the most widely read and cited case studies. It is thoroughly researched, beautifully written, carefully argued. The book contains no provocative central hypothesis or bold new thesis but manages to combine the best of social and narrative history. Byers asks significant questions, assembles a large data base, and then presents the answers and evidence in a pleasing narrative form. This is no easy feat, and doing it as successfully as he does guarantees an appreciative audience of qualitative and quantitative historians as well as large numbers of classroom adoptions.

Byers chose his subject wisely: Nantucket has a past that lends itself to a compelling historical drama. Its very name bespeaks mystery and charm to mainlanders. Successively an independent colony, an appendage of New York, and a town within Massachusetts, Nantucket dominated the Atlantic whaling fleets of the eighteenth century. Led by a Quaker elite and fearful of the power of the Royal Navy, the island tried to remain neutral during the American revolution and suffered depredations from both sides that nearly destroyed its economy. After regaining some of its prewar prosperity and tranquility, Nantucket's economy was clobbered again by a maritime war, and its political stability threatened by the bitter partisan debates of the Federalist and Jeffersonian eras. Several groups of commercial and political leaders flirted with treason during the War of 1812. Byers ends his story in 1820 with the beginning of the economic boom in Nantucket that propelled it and a few other coastal communities into the golden age of whaling. An exciting community—a New England community—Nantucket deserves a chronicler who can tell a good story, and it has one. Happily for scholars, he is also a good social historian.

BRUCE C. DANIELS
University of Winnipeg

JOHN W. TYLER. *Smugglers and Patriots: Boston Merchants and the Advent of the American Revolution.* Boston: Northeastern University Press. 1986. Pp. xiv, 349. $25.00.

John W. Tyler's book successfully challenges accepted interpretations of merchant behavior in Boston in the two decades prior to the American revolution. By carefully examining as many careers of merchants as he can document (some "439 men who, at one time or another made up the merchant community of Boston during the years 1760 to 1776"), Tyler makes a strong argument that the nonimportation agreements were the watershed of political alignment in Boston. By carefully culling biographical details and using hitherto unexamined insurance records, Tyler leads the reader to the conclusion that "patriots" were smugglers who had a broader vision of the future of Boston's trade with the "fringes" of the empire than the British government. They were merchants who were only trying,

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for the most part, to supply some necessities overlooked in the hurried drafting of the nonimportation agreements. They were retailers who resented the efforts of their British suppliers to replace them in their role as American middlemen with less expensive factors (employees), most of whom proved to be staunch loyalists.

In some ways Tyler also reinforces such well-remembered assertions as that attributed to John Adams. Adams argued that one-third of Americans were revolutionists, another third loyalists, and one-third neutral. Tyler is more precise. He argues that 42 percent were active patriots, 30 percent were Tories, and, presumably, 28 percent either were neutral or lacked sufficient documentation for classification (p. 21). Tyler's research also reinforces the model of port town development advanced by Jacob M. Price in "The Economic Function and the Growth of American Port Towns in the Eighteenth Century" (*Perspectives in American History* 8 [1974]: 123–86). In this pioneering essay on eighteenth-century urban development, Price provides a perceptive survey of the factors that both restricted and encouraged Boston's development as a metropolitan center. Tyler's evidence confirms the image of a Boston on the fringes of the empire with no hinterland to speak of and growing competition from innumerable lesser port towns along the nearby coast and elsewhere on the eastern seaboard. According to Tyler, Boston merchants were forced by geographical circumstances and the devastating impact of the early colonial wars to find ways in which to skirt the trading rules of the British empire. All went reasonably well as long as an "accommodation" could be reached with indulgent customs officials, but when London began to seek more stringent enforcement at a time when merchants had overstocked their shelves with goods "patriotism" took root and revolution was all but inevitable.

If all this sounds like a familiar Progressive argument, Tyler would be the first person to agree. His book does not dispute the Progressive interpretation, it seeks to refine it. His goal is to use the fragmentary biographical and insurance evidence to prove that the timing of change was different from what has hitherto been argued. To Tyler the revolution for Boston had its beginnings in the reaction to the Townshend Acts with the nonimportation agreements of 1767–69, not with the Tea Act of 1773 or any other action of Parliament. In what he sets out to do, Tyler succeeds well. His analysis of the inner workings of the mercantile community in Boston in the decades before the revolution is a solid contribution that will stand the test of time. What his book does not do is address the larger issue of the relationship of the Boston mercantile community with the rest of the world. He presents Boston from Boston's point of view (a not unfamiliar problem with Boston and New England historians generally). He does not follow the lead of Price and Thomas M. Doerflinger in attempting to place Boston in the context of all of its trading partners or to see the city's fate in relationship to the success (or failure) of its competition. Doerflinger, in *A Vigorous Spirit of Enterprise: Merchants and Economic Development in Revolutionary Philadelphia* (1986), offers an intriguing thesis of urban growth and mercantile expansion resulting from adversity and high risk taking by entrepreneurial merchants. How were Boston's merchants like or unlike those of London, Glasgow, Philadelphia, New York, Baltimore, or Charleston? How did they interact with, and relate to, merchants elsewhere? Until historians demonstrate a willingness to address these questions in joint studies and through the use of cooperatively developed analytical data bases, it is doubtful that any significant answers will emerge. To expect one historian such as Tyler to do more than he has done is both unfair and ungenerous. For demonstrating what might be done with a comprehensive career analysis of merchants in a number of mercantile communities, coupled with the careful evaluation of what he found in one, Tyler deserves praise and congratulations.

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JOHN PHILLIP REID. *Constitutional History of the American Revolution: The Authority of Rights*. Madison: University of Wisconsin Press. 1986. Pp. ix, 374. $25.00.

This book, a brilliantly executed essay, rich in the use of sources, comes from a legal scholar whose publications in eighteenth- and nineteenth-century American legal and constitutional history have earned respect for their probity and significance. This study not only enlarges on John Phillip Reid's earlier work but also adds a significant contribution to the historical discussion of the antecedents of the U.S. Constitution. Eschewing historiographical modeling and the fashions of contemporary ideology, Reid asserts that eighteenth-century Americans took rights seriously and that therefore the precise meaning of their language properly becomes the "focus of our attention" (p. 132). This viewpoint builds into a major theme, Reid consistently reiterating that "'constitution' did not mean in the eighteenth century what it means now" nor was "constitutional" synonymous with "legal" (p. 7).

Twenty-seven short chapters divide into three major parts: the identification of rights, the articulation of the authority for rights, and the discrete actions prompted by the capacity created in rights. The central part receives Reid's greatest attention.

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He considers that historical works that stress intellectual, political, and economic factors overlook the significance of the theoretical sources for rights insofar as they maintain that American whigs were concerned with constitutional ideas merely as utilitarian political tools and were therefore unable and unwilling to define rights with precision. On the contrary Reid establishes how accurately Americans articulated customary rights, and, most significantly, he explains the connection they made between the authority for rights and how this knowledge forged a capacity for the defense of those rights. Within this context his vivid discussion of property, migration, original contract, and charters reveals a constitutional portrait of Americans at once intellectually complex and politically mature. Reid treats such popular historical subjects as John Locke, natural law, salutary neglect, and the Declaration of Independence with an incisive legal perspective that yields challenging comments about the historical relevance of several of these time-honored considerations.

Reid's central argument, however, is reserved for his contentions about how the American revolution occurred within the British imperial constitutional framework. Crucial is his assertion that the eighteenth-century British constitution itself was at a vital crossroad between the old constitution of "customary powers, with rights secured as property" and the newer constitution "of sovereign command and of arbitrary parliamentary supremacy" (p. 229). The conflict between the two was profound and ultimately irreconcilable as the Americans, with occasional misgivings and uncertainties, sustained the old and Parliament lurched toward the new. Finally, the Americans, facing a formidable constitutional dilemma regarding the preservation of customary rights and the reach of arbitrary power, resolved the matter in such a way as to establish a new constitutional framework for sovereignty. That Reid spells out with such clarity the various traditional sources for that sovereignty gives this book a compelling intellectual force that deserves the closest scrutiny.

Reid believes that Americans defined and asserted rights as exclusively "English" and maintained that they "enjoyed these rights even more extensively" than did their English counterparts (p. 14). A variety of eighteenth-century American comments might cast doubt on such a single-dimensional view. Before the reflexive response of Americans to Parliament during the 1760s, there developed distinctive American traditions, customs, laws, and constitutional precedents. In colony after colony this experience lent special meaning to the Englishness of rights. The eighteenth-century British constitutional controversy, then, sparked a recognition of American history, of American space, and of American constitutional distinctiveness. Given that the publisher announced this book as the first part of a trilogy, Reid may intend to discuss at greater length a variety of subjects, including the transformation of the American constitutions. Just as it stands, however, Reid's present book is a distinguished contribution to American constitutional history.

GEORGE M. CURTIS III
Hanover College

RICHARD B. MORRIS. *The Forging of the Union, 1781–1789*. (New American Nation Series.) New York: Harper and Row. 1987. Pp. xiv, 416. $22.95.

Richard B. Morris's book represents an important contribution to the bicentennial bookshelf, summarizing and developing an array of themes explored by the dean of revolutionary-era historians during a distinguished career. Particularly useful are strong chapters on the nation's "first depression" and on "friction and frustration" in diplomacy. Morris invokes problems in both areas to explain the drive for a stronger union: "a prolonged business slowdown" (p. 134) was exacerbated by British refusal to conclude a commercial treaty favorable to American interests. Morris agrees with John Jay—whose unpublished papers he is currently editing—that "British dumping and trade exclusion" were graver threats to the new nation than western separatism, boundary disputes, "or even domestic insurrection that lay ahead" (p. 205). Shays's Rebellion and agrarian unrest throughout the states did, of course, create "a crisis atmosphere" that played into the hands of nationalist reformers (p. 266). The concluding chapters provide a concise narrative of their campaign to strengthen the union, including an excellent brief account of the Constitutional Convention.

Although Morris is a thoroughgoing "nationalist," his recognition of the limits of American egalitarianism supplies a salutary corrective to the book's otherwise celebratory tone. His sympathetic treatment of the "wave of backcountry resistance to debt recovery and tax collection" (p. 264) is in the best progressive mode, as are indictments of the Framers' unholy compromises with slavery. Yet if postrevolutionary America was by no means an "egalitarian society," Morris concludes, "reforming idealism was heading in that direction, swiftly for some, haltingly for others" (p. 9).

The major difficulty with this latest addition to the now venerable New American Nation series is that Morris's well-rounded portrait of confederation period society and politics is not adequately connected to his treatment of the constitutional reform movement. Over the last generation, historians have sought to put the founding into context by studying

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the reformulation, dissemination, and reception of political ideas, but Morris's attention to these questions—and to the vast literature on ideological change—is perfunctory at best. He instead adopts an anachronistic, legalistic approach to issues that contemporaries found highly problematic. In an early chapter entitled "Congress and the People," Morris argues unconvincingly that the nation preceded the states, concurring with Justice Joseph Story that the union was "spontaneously formed by the people of the United States" (p. 57). Morris also posits, in prefatory comments that are not subsequently adequately developed, a "burgeoning sense of American nationality" (p. vii). The "nation" already existed as a legal fact and as a fact in the popular mind, he thus suggests, well before reformers launched their campaign to perfect the union—and notwithstanding the legendary "imbecility" of congressional government under the Articles of Confederation. The result is that there is no real drama in Morris's version of the constitutional reform movement or ratification contest: in effect, the conclusions were foregone. The Framers simply had to restore the faltering union to its true nationhood and convince skeptical voters to acknowledge their natural loyalties.

The problem with this account of the founding is that it fails to recognize the full range of historical possibilities. It also does not allow for the development of key ideas—such as "union" itself—over time. But these shortcomings do not diminish the value of Morris's impressive volume. Students of the period will long be grateful for his authoritative discussions of so many crucially important issues.

PETER S. ONUF
Southern Methodist University

DANIEL GEORGE LANG. *Foreign Policy in the Early Republic: The Law of Nations and the Balance of Power.* (Political Traditions in Foreign Policy Series.) Baton Rouge: Louisiana State University Press. 1985. Pp. 175. $20.00.

In the 1950s and 1960s scholars of the early American republic spent considerable time and energy trying to understand the founding fathers by examining the books the men had read. Presumably, familiarity with the ideas of seventeenth- and eighteenth-century authors would help historians comprehend the events of the 1760s and 1770s; H. Trevor Colbourn's monograph, *The Lamp of Experience* (1965), is an example of this approach to history.

Charting the new nation's foreign policy in the turbulent era of the French revolution, the revolutionaries-turned-statesmen frequently resorted to using the past, citing the writings of, among others, Hugo Grotius, Samuel Pufendorf, and, most notably, Emmerich de Vattel. Daniel George Lang's study is concerned with the ideas of these international theorists and how their thinking was used by early American politicians, Alexander Hamilton and Thomas Jefferson being the most important, in the 1790s.

According to Lang, Grotius, John Locke, and Vattel "rejected religious motives as a just cause for war," shifting "just-war thinking" to "secular foundations" (p. 4). Of signal importance in the transition was Vattel's *Law of Nations* (1758), which created a new synthesis, combining "the universalism and categories of the just-war traditions with aspects of modern natural rights theory derived from [Thomas] Hobbes and praise for the balance of power system" (p. 10). Lang maintains that the Vattelian tradition has deeply influenced American foreign policy; thus, American politicians and diplomats resort to it when they must articulate the nation's foreign policy.

Certainly, Hamilton, Jefferson, and James Madison referred to Vattel in the foreign policy disputes that marked the Federalist era, in the Nootka Sound crisis, at the outbreak of the Anglo-French war in 1793, over the Jay Treaty, and then during the undeclared war with France at decade's end. Indeed, whether Federalists or Republicans, the statesmen of the infant United States could have proclaimed "we are all Vattelians" for they understood the linkage between international law and the balance of power.

Although Lang is convincing in establishing Vattel's presence in the communications of American diplomats and political leaders, his book has a variety of shortcomings. On the minor side he makes little errors such as designating U.S. envoys as ambassadors when the United States did not employ the rank until the 1890s, and he erroneously places a circumflex over Edmond Genet's name. More important, Lang should have given more than the cursory treatment he does to the foreign policies of Jefferson and Madison, and he should have read more widely in the literature of early American diplomacy; his book would have had a more authoritative ring had he done these things.

CLIFFORD EGAN
University of Houston

WILLIAM G. MCLOUGHLIN. *Cherokee Renascence in the New Republic.* Princeton: Princeton University Press. 1986. Pp. xxii, 472. $29.50.

William G. McLoughlin's book is a "study of the rise of romantic nationalism in America, told in terms of the struggle of the Cherokee people to accept the promise of equal citizenship in the new nation" (p.

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xv). The author traces the Cherokees' attempt to accommodate themselves to the revolutionary changes brought on by increasing contact with their American neighbors from the 1790s to the 1830s. Revitalization through political, economic, and cultural change became a way to maintain their heritage and their lands. Held out to them initially was the Jeffersonian ambition that "We shall all be Americans." But it was not to be. The vision of Thomas Jefferson was not the reality of Andrew Jackson.

This is more than a chronicle of events in Cherokee history during this decisive period, although the material covered here has not been better treated before. McLoughlin includes not only the tribe's relations with the federal government but also the internal divisions that seemed likely to split the nation on several occasions. He expertly describes the subtleties of political interplay between the Cherokees and federal officials and between conservative and progressive Indian contestants.

The Cherokees succeeded in achieving the ideals of Jefferson: to become "civilized," to give up hunting, and to take up agricultural pursuits. It was not an easy transition. This is the story of a sporadic and controversial conversion to new ways. McLoughlin presents the transitional period in greater detail and with keener insight than we have had it before. The internal struggles among the Cherokees to reach Jeffersonian aims were marked by ideological, generational, and geographical divisions. Persuasive individuals in the nation became convinced (some through bribes and threats, others for altruistic reasons) that acculturation was possible and desirable and led their fellow tribe members in that direction. The irony was that, as the Cherokees worked toward Jeffersonian promises, the external rules and rulemakers changed.

For McLoughlin the "essence of Cherokee renascence was to establish a distinct national identity, firmly grounded in economic self-sufficiency and political self-determination" (p. 277). But, with the federal government holding the purse strings and unaccepting of Cherokee statism, there was little hope that true revitalization could occur. By the 1810s the Cherokees had an "ebullient expectancy of successful participation as equals in the future destiny of the republic" (p. 451). By 1833 those hopes were dashed against the realities of white ethnocentrism and racism, growing state sovereignty, federal resistance, and internal division.

McLoughlin's book should find a well-deserved space beside other distinguished books on Cherokee history.

GARY E. MOULTON
University of Nebraska

ROBERT E. BIEDER. *Science Encounters the Indian, 1820–1880: The Early Years of American Ethnology.* Norman: University of Oklahoma Press. 1986. Pp. xiii, 290. $19.95.

This is a synthesis of the nineteenth-century intellectual arguments that attempted to explain the origins and cultural context of the American Indian. Author Robert E. Bieder skillfully uses the correspondence and publications of five key figures in the history of American ethnology—Albert Gallatin, Samuel G. Morton, Ephraim G. Squier, Henry Rowe Schoolcraft, and Lewis Henry Morgan—as the vehicle for explicating the various schools of thought. Gallatin, a philologist and one-time secretary of the treasury, was a product of the Enlightenment who believed in the perfectibility of humanity. To him the Indian could be moved from his "savage" origins to "civilization" through education and an improved environment. Morton, a physician and phrenologist whose views reflected the racial concerns of his day, believed "science" proved the basic inferiority of the Indian. Squier, a pioneer archaeologist and "lay" ethnologist, made his name studying the ancient mound builders. Convinced that the mounds were the product of a superior race of Indians, he believed, in contrast to Morton, that the Indians had once attained a high civilization and could do so again, and he tried, albeit clumsily and unsuccessfully, to bring competing theories together. Of the five ethnologists who are the focus of this study, Schoolcraft, Indian agent and author, had the most direct experience with native Americans and, in many ways, prefigured modern ethnologists who base their findings on field work. His ethnology, however, was shaped largely by his staunch religious beliefs and therefore kept him from rising above the intellectual constraints of his day. Morgan, although considered the "father" of American anthropology, was an intellectual bridge to the age of Enlightenment and firmly within the evolutionary tradition. He was, Bieder writes, "a keeper of the faith, a keeper of the Enlightenment's faith in man" (p. 246).

Bieder has done more than write a series of biographies. The writings of each of these men, analyzed in the intellectual context of their day, serve as the framework for explaining the development of evolutionary thinking with respect to ethnology. But that is not the only theme to be found in this useful summary. Bieder discusses the debate between monogenists and polygenists as well as the arguments over evolution and degeneration, the issue of biology versus culture in physical anthropology, and the rivalry between "field workers" and "armchair" theoreticians.

This was an ambitious undertaking, which required the assimilation and distillation of an im-

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mense body of literature. "Its emphasis," Bieder notes, "has been to examine a period little noted in the history of American anthropology, to cast greater light on the totality of the period's ethnological achievements, and to underscore how American Indians and their cultures buttressed the early development of American ethnology" (p. 251). Bieder has done this well. His writing is lucid; his scholarship sound. His book will become required reading for college anthropology courses, especially those relating to ethnology.

HERMAN J. VIOLA
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RICHARD E. ELLIS. *The Union at Risk: Jacksonian Democracy, States' Rights, and the Nullification Crisis.* New York: Oxford University Press. 1987. Pp. xi, 267. $32.50.

In his excellent first book, *The Jeffersonian Crisis: Courts and Politics in the Young Republic* (1971), Richard E. Ellis demonstrated that constitutional crisis in the early republic had political consequences beyond the Constitution. In the present book Ellis pursues the same strategy for the Jacksonian era. He argues that the nullification controversy, the great constitutional crisis of Andrew Jackson's presidency, helped define the nature of Jacksonian Democracy. Ellis's intriguing irony is that the new Jacksonian definition represented a major defeat for Jackson.

Ellis's major protagonists are not nullifying states' righters versus antinullification nationalists but rather traditional states' righters versus an ominous new states' rights breed. Jackson was the old sort of states' righter, seeking majoritarian democracy and perpetual union through limited federal government. John C. Calhoun was the new variety, seeking minority protection and perpetual slavery through nullification or secession. Jackson, caring little about tariffs and passionately about majoritarian union, declared in his Nullification Proclamation eternal war against law negators and union busters.

Ellis calls this appeal a loser in the South. His new material conveys a dense evocation of the southern Jacksonian realm beyond South Carolina, possessing some sympathy for Calhoun and less for Jackson on certain issues. These southern Jacksonian states' righters, while opposed to the doctrine of nullification, thought slavery was at risk if Jackson smashed the doctrine of secession. More militant states' righters also thought Jackson far too unmilitant on tariff reform. Because of division in his southern home base, Ellis's Jackson lost control of events. Calhoun and Henry Clay wrote tariff reform, Jackson's Force Bill only passed after force would no longer be necessary, and new states' righters began their ultimately successful effort to define Jacksonian states' rights as proslavery stuff.

As one who has written about nullification in a narrower way than Ellis, I admire his book for widening the scope of inquiry. We have always known that southern Jacksonians were restive about the Nullification Proclamation. But previous focus on what ailed South Carolina has obscured the importance of this broader malaise. Ellis's fine portrait of the non-Carolina local level illuminates why Jackson momentarily lost control at the national level and why southerners would soon impose the new definition of states' rights on the national Jackson movement.

Ellis's problems come because his wider focus remains too narrow. His conflation of Jacksonian Democracy and states' rights misses a central impact of nullification: the way the crisis helped produce a southern states' rights Whig movement. Many of Ellis's anti-Jackson Jacksonian stars were precisely the luminaries who switched parties rather than seek to make Jacksonianism a states' rights movement. Ellis's focus on the nullification crisis also obscures the fact that later crises, especially over gag rules and Texas annexation, had much more to do with the redefinition of Jacksonian Democracy. The states' rights impact of nullification was both more widespread than here portrayed, because it was bipartisan in nature, and less decisive than here claimed, because it was too early to create the ultimate concept of Jacksonian Democracy.

Still, Ellis has convincingly portrayed precursors of what was to come. By successfully urging once again that political historians stop viewing constitutional history as some separate field, Ellis has placed the South, states' rights, and slavery right where these commodities belong—at the center of inquiry into the Jacksonian puzzle.

WILLIAM W. FREEHLING
Johns Hopkins University

KEITH J. HARDMAN. *Charles Grandison Finney, 1792–1875: Revivalist and Reformer.* Syracuse, N.Y.: Syracuse University Press. 1987. Pp. xvii, 521.

A full-scale life and times of Charles Finney has long been a major desideratum. With his innovations in popular evangelism and his importance in pushing mainstream American religion beyond Calvinism, Finney is immensely important for religious historians. As the embodiment of the link between popular revival, social reform, and the democratic ideology of the antebellum North, he is equally significant for American cultural historians. Several scholars (including David Weddle, Leonard Sweet, James Johnson, and Timothy L. Smith) have ably illumi-

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nated parts of Finney's career, but until now there has been no satisfying biographical synthesis. Keith J. Hardman's long and fact-filled book, which makes especially good use of the Finney papers in the Oberlin College Archives, still leaves questions about Finney's "times," but it is the best study to date of his important "life."

Hardman's mastery of the sources allows him to develop a convincing portrait of Finney's character as serious-minded and ethically rigorous. Not for him was the blithe pursuit of Mammon that has come to characterize our contemporary television evangelists. In 1837, shortly after giving up a large New York church for the duties of a theology professor at Oberlin, Finney was so strapped for funds that he was forced to sell "my travelling trunk, which I had used in my evangelistic labors." His response was simply to pray and to conclude "that, if help did not come, I should assume that it was best that it should not" (p. 339). Hardman himself is of two minds about Finney. He appreciates Finney's sincerity, determination, and hard work. But he also finds the revivalist's *Memoirs* self-serving and uncharitable. In addition Hardman does not like the theological stress that Finney placed on the unaided exertions of the human will ("natural ability run rampant" is how he labels it [p. 345]). Yet Hardman does set out the major events and stages of Finney's career with scrupulous attention to detail—from his dramatic conversion (1821), his early preaching in upstate New York (1820s), the famous meeting at New Lebanon, New York, with ecclesiastical opponents from New England (1827), the great revival in Rochester (1830–31), his spiritual invasion of Philadelphia, Boston, and New York (early 1830s), his role in the abolitionist disruption of Cincinnati's Lane Theological Seminary and his transfer to Oberlin (both mid-1830s), and his preaching tour in England (1850), to his last crusade against the Masons (1860s). And he is equally informative on Finney's major writings and their extensive influence (*Lectures on Revivals* [1835], *Systematic Theology* [1846–47], *Memoirs* [1876]). Readers who do not share Hardman's judgments, or who would like to see such judgments rendered with greater nuance, will still thank him for the thorough presentation.

For all its strengths, Hardman's book still leaves much to be said about the meaning of Finney for his times. The biography details Finney's associations with the most dynamic social reformers of his day, especially Theodore Dwight Weld but also John Humphrey Noyes, Lewis Tappan, Arthur Tappan, and William Lloyd Garrison. It also shows his intimate involvement in the disputes that split the antislavery phalanx of the 1830s. And it demonstrates Finney's importance as a major force at Oberlin College when American higher education could still be both determinedly Christian and socially progressive. Hardman, however, sticks to his useful facts and only occasionally ventures assessment of Finney's place in the larger world. The result is a helpful book, but one lacking the contextual depth of Bertram Wyatt-Brown's study of the Tappans or the interpretive creativity of Paul Johnson's account of the Rochester revival.

MARK A. NOLL
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PAMELA HERR. *Jessie Benton Fremont: A Biography*. New York: Franklin Watts. 1987. Pp. xiii, 496. $24.95.

Pamela Herr's biography of Jessie Benton Fremont presents a commendable story that rivals works of the imagination—a story of a woman long neglected by historians. More than a biography, the present book is reminiscent of earlier eras when historians wrote good stories about "life and times." It relates a unique relationship between Jessie and her father, the formidable Thomas Hart Benton, and it is meant to be a tale of an odd marriage to a controversial figure, John Charles Fremont, described as hero, fraud, and philanderer.

Succinctly and sprightly, Herr captures much of the spirit of this exceptional woman who exhibits a curious blend of will and wit, passion and passivity. Her life seems an expression of extremes. She enjoyed enormous wealth but ended her days bereft of fortune, forgotten, and dependent on her wits and the charity of the few who remembered. She took delight in the cheering crowds, the accolades of party leaders and press, only to be scourged as "General Jessie," the female politician who planned political and military campaigns, assailed President Abraham Lincoln, and by so doing stepped beyond the pale of hearth and home.

Herr sets this saga of stirring successes and devastating defeats in the climate of the times. Jessie's story unfolds amid the truculent antebellum political world in which principles frequently bowed before ambitions. The major issues and figures who brought the nation to disruption touched Jessie's life, and Herr's sense of these times is as sound as her research. Unfortunately, this work is weakened by its failure to include many sources in the too few citations.

The rich and varied life of Jessie Fremont gave the author all the elements of good biography, and her research uncovered valuable new materials that have removed Jessie from the shadows of father and husband. Yet this work leaves much untold. Herr gives only glimpses of a woman whose character is a maze of contradictions—contradictions the author too often attempts to explain by taking refuge in the subjunctive. Speculation and presentism cannot reconcile Jessie's bold sorties into the public sphere with

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her humble retreats into the domestic realm. Still, the character and personality of Jessie Fremont are elusive and will remain so until the silence is broken concerning her relationship with her husband, who seems to have called forth Jessie's greatest strengths and her most debilitating weaknesses.

CAROLYN E. DELATTE
McNeese State University

THEODORE ROSENGARTEN. *Tombee: Portrait of a Cotton Planter; With the Journal of Thomas B. Chaplin (1822–1890).* Assisted by SUSAN W. WALKER. New York: William Morrow. 1986. Pp. 750. $22.95.

For readers acquainted with Willie Lee Rose's 1964 study *Rehearsal for Reconstruction*, the name Thomas B. Chaplin will have a familiar ring. Owner of "Tombee," a medium-sized cotton plantation on St. Helena Island, South Carolina, Chaplin was also an inveterate diarist whose manuscript journal for the years 1845–61 afforded Rose a crucial window on the slave experiences that helped shape black responses to Federal troops and Yankee missionaries during the Civil War. Unpublished until now, an edited, annotated, and substantially abridged version of Chaplin's diary fills nearly four hundred pages of the present volume, which begins with Theodore Rosengarten's 317–page biography of Chaplin and concludes with detailed genealogical charts and tabulated data on the land, slaveholdings, and farm produce of selected St. Helena planters.

Reminiscent in some respects of traditional "life and letters" compilations, the present work avoids the filiopietism of its genre and achieves high editorial and literary standards. *Tombee* does not, however, entirely escape the confusion of intellectual purpose created by juxtaposing first- and third-person accounts in a single book. Functioning as both author and editor, Rosengarten seeks to explain Chaplin and to allow him to explain himself. The two endeavors are not always complementary, and when conflicts arise the editorial imperative frequently takes precedence. Rosengarten concedes as much when he describes the biography as a "framework for organizing the [diary's] seemingly random notations" (p. 11).

Neither component of the book would readily stand on its own. The journal dominates the volume, dictating both the chronological focus and the thematic emphasis of the biography. Judged on its own merits, Chaplin's diary ranks as a valuable, if less than earthshaking, addition to the already long list of published letters, journals, account books, and reminiscences that chronicle lowcountry plantation life during slavery's final decades. Chaplin's journal provides interesting glimpses of upper-class social and domestic life and sets down the essential points of daily plantation routine. In most respects the patterns are familiar, involving minor variations on the themes of paternalism, patriarchy, and conspicuous consumption. Chaplin experimented with seed selection and improved methods for cultivating corn, but even these efforts can best be understood as holding actions intended to offset continued declines in cotton production and thus perpetuate the status quo. Innovation was not the diarist's strong suit. In most areas the master of Tombee displayed a conventional turn of mind and seemed content to let plantation affairs continue in a well-worn path. He monitored the work output of field hands, recorded slave births and illnesses, and ordered specific punishment for cases of serious misconduct. Largely oblivious or indifferent to the domestic life of his human chattels, he willingly delegated overseeing duties to his black driver Robert and often left home to seek company or diversion while the field work was underway. His diary accurately reflects these priorities and is written with considerable frankness and candor. It is not, however, an intellectually compelling document.

Rosengarten's topically organized and sparsely footnoted biography examines such themes as the family, social life, agriculture, slavery, health conditions, and the Civil War. The author believes that Chaplin "typified his social class" (p. 27) but admits that much about the diarist was unrepresentative even of other St. Helena planters. Descended from prominent local families, Chaplin married at age seventeen and immediately took over Tombee, the boyhood home he inherited from his deceased father. Economic disaster struck quickly, and by 1847 some forty of Chaplin's slaves had been sold to cover debts, a blow from which he was unable to recover even in the boom times of the next decade. Kept solvent by periodic gifts from his wealthy mother, young Thomas lived beyond his means and spent freely to gain the approval of neighboring gentry whose ridicule he had valid reason to fear. Active in the St. Helena Agricultural Society, he held no strong sectional views but enjoyed the socializing and emotional excitement of political speeches. The war brought sweeping land confiscation and economic ruin to Chaplin and virtually all other planters whose estates fell within the boundaries of the famed "Port Royal experiment" in emancipation and free labor. Always prone to fits of depression, Chaplin lived from hand to mouth after 1865. He rented land for a short time, taught briefly in a freedmen's school, and succumbed temporarily to opium addiction during the 1870s. Bitter and ironic postwar notations in the antebellum journal attest to Chaplin's troubled state of mind as he struggled against seemingly hopeless odds to recover his lost estate. Finally, in 1890, he was awarded legal title to nearly all of Tombee but died nine months later

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without taking physical possession of the land. Readers conversant with recent scholarship on southern planters before and after 1865 may discover much in Chaplin's story that runs counter to the experience of other slaveholders, including lowcountry planters outside the Port Royal area. Whether or not Rosengarten has succeeded in establishing his subject's typicality, few will question the judgment that "loss and disappointment" constituted the central themes of Chaplin's life (p. 25).

CLARENCE L. MOHR
Tulane University

STEVEN M. STOWE. *Intimacy and Power in the Old South: Ritual in the Lives of the Planters.* (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1987. Pp. xviii, 309. $29.50.

Nearly fifty years ago Caroline F. Ware, introducing *The Cultural Approach to History*, argued that, since every part of a culture is related in some way to every other part, it was time for historians to try looking at the whole, drawing on concepts from anthropology and linguistics, among other fields. She could not have guessed that one-half century later one historical monograph after another would announce its debt to Clifford Geertz, Mary Douglas, and Victor Turner in pages filled with "thick description." Steven M. Stowe's *Intimacy and Power* is another such book, evidence again that the walls between disciplines are becoming more permeable every day. In spite of the millions of words that have been devoted to the relatively small group of families that made up the antebellum planter class (using the customary "twenty slaves or more" definition), there seems to be no diminution of interest in finding new ways to probe their psyches.

Stowe assumes that if he can reconstruct the intellectual and emotional world of the planters he will be able to explain their ability to dominate a society in which they were a small minority. He proposes to make his way into that world through a close examination of three rituals: dueling (or "affairs of honor"), courtship, and the passage to adolescence. In each case an examination of the rules of the game as laid down in advice books and manuals is followed by detailed attention to letters and diaries as a way of reconstructing real life experience. A final section of the book uses the personal documents of three quite different Carolina families to illuminate certain general aspects of elite experience. The real life data are engrossing and could stand on their own as a valuable addition to our knowledge of the inwardness of planter family life. Stowe draws on anthropological theories of ritual for his interpretations of words and behavior, interpretations that might startle the subjects of this analysis if they could read them. There is very little free will and not much room for individual quirks of mind in Stowe's narrowly defined planter group. (All his data are drawn from the Southeast; he offers no evidence from the world of Daniel Hundley's cotton snobs or the men who populate Joseph Baldwin's *Flush Times in Alabama and Mississippi* [1853]). Every southerner is a prisoner of the culture, and every word and deed is more than it seems.

Geertz himself once wrote that "nothing has done more, I think, to discredit cultural analysis than the construction of impeccable depictions of formal order in whose actual existence nobody can quite believe." Overinterpretation is the hazard for this kind of historical writing. The danger is always that life will be made to appear less chancy or contingent than it customarily is and that people's words will always be assumed to reflect accurately their deepest feelings and assumptions, including those of which they are unaware.

It is manifestly impossible for a brief review to do justice to this intricately woven book, filled as it is with intriguing detail that, whatever it reveals about the collective consciousness, is guaranteed to engage the interest of any committed historian of the South. Stowe's argument makes fascinating reading, but is it true? Is it even history? Up against a word limit I can only say, read it yourself; however you answer these questions, you will not be bored.

ANNE FIROR SCOTT
Duke University

FRED ARTHUR BAILEY. *Class and Tennessee's Confederate Generation.* (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1987. Pp. x, 205. $21.00.

Between 1915 and 1922, two Tennessee archivists developed a questionnaire that they distributed to their state's surviving Civil War veterans. Each questionnaire solicited a brief autobiography and responses to specific queries about the South before, during, and after the war. The archivists expected the survey to silence charges that the South had been dominated by planters who oppressed nonslaveholding whites and to confirm claims that the southern social order was for whites seamless and harmonious. Some of the twelve hundred fifty old soldiers whose responses Fred Arthur Bailey analyzes remembered unity and equality, but a significant number gave evidence of social hierarchy, tension, and resentment.

The veterans portrayed a society in which ownership of land and slaves made a difference. Their descriptions of housing, work, leisure, education,

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officeholding, and more demonstrate that although poor whites, plain folk, and planters often lived in the same neighborhood they lived very different lives. Even more valuable is the unprecedented evidence the veterans provided of how white southerners felt about those differences. Almost half of the responses came from poor whites and yeomen, for whom first-person testimony is rare. Slaveholders and nonslaveholders often remembered the social structure and class relationships differently. One poor farmer declared that "all who owned as much as one negro seemed to feel that they were in a separate and higher class than the common people" (p. 20). Another claimed that "Negroes and poor white men did the work," while slaveholders spent their time "hunting and fishing and riding around" (p. 69). A third said that planters "didn't treat white men any better than slaves" (p. 41). Slaveowners were more likely to remember friendly relations between classes of whites, which suggests perhaps that the common folk usually kept their opinions to themselves. Certainly, resentment of aristocratic privilege and airs did not develop into a significant antiplanter or antislavery movement at the ballot box or elsewhere.

The Tennessee Civil War Veterans Questionnaires were largely overlooked until the 1980s when Bailey began to mine them for several articles and this significant book. He provides a careful summary and an astute analysis comparable to *Slavery Remembered* (1979), Paul D. Escott's assessment of the Federal Writers' Project Slave Narratives. But it would be an exaggeration to say that the questionnaires do for poor and yeomen whites what the slave narratives do for slaves. The questionnaires are neither as extensive nor as rich, but they do give voice to nonslaveholding whites. They allow us to see nineteenth-century southern society through their eyes, even though the vision might be a little impaired after one-half century. Recently published in five volumes, the questionnaires are a major historical source, and this book is a valuable explication of them.

JAMES L. ROARK
Emory University

TERRY L. JONES. *Lee's Tigers: The Louisiana Infantry in the Army of Northern Virginia*. Baton Rouge: Louisiana State University Press. 1987. Pp. xiv, 274. $22.50.

Of all the elements that composed Robert E. Lee's Confederate army, none were more flamboyant or rambunctious than the units from Louisiana. "Wharf rats from New Orleans" and the "lowest scrapings of the Mississippi" were some of the kinder assessments made of them. At least three of the units adopted the title "Louisiana Tigers," and each seemingly vied with the others in disrupting civilization.

For example, en route by rail to Richmond in 1861, the men in G. A. G. Coppens's First Louisiana Zouaves uncoupled the officers' car and then hijacked the train. They were ransacking Montgomery, Alambama, when irate officers, plus the First Georgia Volunteers, brought them under sullen control. Once in Richmond, a resident there wrote, the Louisianians "roamed about the city like a pack of untamed wildcats." As long as they were there, "thieving, burglary, and garroting in the streets" were nightly occurrences.

The Louisianians could erect a chapel with the same precision with which they could wreck a saloon. Their misbehavior became celebrated, but so did their valor. In such engagements as Kernstown, Second Manassas, and Spotsylvania, the participation of the "bayou brawlers" was pivotal. At Chancellorsville General Jubal Early became so excited at their valor that he shouted: "Those damned Louisiana fellows may steal as much as they please now!" By war's end the Louisianians had the unique distinction of the highest percentage of battle losses, as well as the highest desertion rate, in the Army of Northern Virginia.

Terry L. Jones has done a praiseworthy job in chronicling the experiences of the various Louisiana units in Lee's army. Not all units were as reckless and as colorful as Roberdeau Wheat's battalion or Coppens's Zouaves, but Jones has adroitly managed to bring all of the Louisiana regimental accounts together and treat them as a unit. Using untapped manuscripts in depositories from New York to Texas, consulting all pertinent printed sources, and relying heavily on soldiers' letters and diaries, Jones has re-created the saga of the "Tigers" by allowing the men—whenever possible—to present their own stories and sentiments.

Jones's account is both detailed and analytical. The vandalism of the Louisiana troops in 1861–62 in the main became heroism in the last three years of the Civil War. Jones is on firm ground when he concludes: "The Louisiana Tigers were the Dr. Jekyll and Mr. Hyde of the Confederacy. They were drunken, lawless renegades who often posed a greater threat to the South's civilians than did the Yankees . . . Yet when it came fighting, the Tigers were rated among Lee's most dependable soldiers" (p. 229).

Incidentally, the Louisiana State University Press deserves the thanks of the historical profession here. How refreshing it is to see once again footnotes at the bottom of each page!

JAMES I. ROBERTSON, JR.
Virginia Polytechnic Institute and State University

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GAINES M. FOSTER. *Ghosts of the Confederacy: Defeat, the Lost Cause, and the Emergence of the New South, 1865 to 1913*. New York: Oxford University Press. 1987. Pp. x, 306. $29.95.

There has been interest of late in how the South coped with defeat. Thomas Connelly has blamed condescending Virginians for seizing control of memory and elevating Robert E. Lee into the god of myth. Charles Reagan Wilson has blamed the priests for sanctifying the Lost Cause and thereby a reemergent social order. No one seems happy with what was done, being convinced that myth marred progress, although opinions differ on the merit of progress and so the culpability of the mythologists.

Gaines M. Foster disdains the imprecisions of "myth" and "civil religion" and instead offers the suggestion of a chronology and the hint of a social context. First came grief and local associations to memorialize the dead in their cemeteries; then in the 1870s a "Confederate revitalization movement" in Virginia, composed of veterans of the officer corps, which failed to extend itself beyond its class and state and practiced apologetics; finally a popular movement only in the late 1880s, with the United Confederate Veterans and the Sons and Daughters of the Confederacy. These last reached a wider southern and class audience by balancing the claims of honoring the dead and their cause against the necessity of sectional reconciliation and industrial regeneration. Traditionally, Foster stresses the Spanish-American War as sealing the bargain between North and South. Less traditionally, he sees all this as a temporary episode, not the underpinning of a persistent postbellum social identity.

The analysis is convincing, though repetitively argued and perhaps too rigorously confined to the Confederate aspect of the Lost Cause. There is a tendency to conflate organizational history with *mentalité*. Most regrettably, Foster misses the opportunity to compare the South with other defeated cultures, while indicating in a few garbled paragraphs his awareness of the possibility. That much was occasioned by contemporary social tension seems plausible, although Foster asserts the point more than he proves it. But Foster is intriguing on the iconography of Confederate art, and he has been unusually thorough in his research, reaching down even into the graveyard of masters theses.

Foster makes clear that the sharp bereavement of 1865 was to modulate by 1913 into the social cheeriness of evening balls and parades. One is led thereby to suspect that the most lasting legacy of the Confederate dead may be the bizarre processions that still roll through southern towns. Foster has a photograph of a Confederate parade in Richmond in 1907, where the participants seem already to have the Ruritanian uniforms that decorate modern adolescent and student musicians. Can even the baton twirler be blamed on the tardiness of the unfortunate Pickett?

MICHAEL O'BRIEN
Miami University
Oxford, Ohio

RUTH CURRIE-MCDANIEL. *Carpetbagger of Conscience: A Biography of John Emory Bryant*. Athens: University of Georgia Press. 1987. Pp. 238. $30.00.

The recorded history of Reconstruction in the South is so laden with stereotypes that modern historians have been even more reluctant than usual to formulate generalizations about the period and its participants. Yet, if one could draw a picture of a "typical" carpetbagger, that person might bear a close resemblance to John Emory Bryant of Georgia. Much of what Ruth Currie-McDaniel tells us of this man fits with what we already know about carpetbaggers, either from other biographies or from group studies.

Bryant was born and educated in Maine; his father was a Methodist minister. Young Bryant became active in temperance reform and developed a life-long faith in the power of education to transform society. He opposed slavery and was an early member of the Republican party. When the Civil War began, Bryant entered the Union army and was soon commanding black troops in the sea islands of Georgia and South Carolina. After the war ended, he remained in Georgia as an agent of the Freedmen's Bureau. At least a year before the passage of the first Reconstruction Act, Bryant was at work organizing the blacks of Georgia to work for equal rights and the vote. He was closely involved in the organization of the Republican party in the state, served in the state constitutional convention in 1867, and was elected to the first legislature under the new constitution. Bryant remained active in the state Republican party until he left Georgia for good in 1888.

Much of Bryant's life was a study in frustration. He was born into poverty and throughout his life struggled to make ends meet, yet he was never able to provide adequately for his wife and daughter. He tried teaching and law but preferred a political career, both for the income it would bring and the opportunity it would provide for achieving racial justice in Georgia. Yet after his one term in the state's legislature he never again held an elective office, and when he left Georgia most of his hopes for achieving equality for blacks in the state had been dashed.

Currie-McDaniel sees in Bryant the mixture of personal and idealistic motives that she believes characterized many carpetbaggers, as well as Recon-

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struction politics in general. On balance she argues convincingly that Bryant was a dedicated reformer, a "carpetbagger of conscience" who remained true to the causes of temperance, educational reform, and, especially, racial justice throughout his life. She also acknowledges that he had numerous personal faults, including a contentious personality and a sometimes arrogant self-righteousness that got him into constant scrapes with parents of his pupils, fellow army officers, Freedmen's Bureau officials, and members of the Georgia Republican party.

Since Bryant was the only carpetbagger of significance in Georgia politics during Reconstruction, one would have hoped his biography would provide more insight into the turbulent workings of the state Republican party. Currie-McDaniel is able to add little, however, to the already dreary story of intraparty factionalism that helped doom the party in the state. Currie-McDaniel does have some interesting information on the relationship between Bryant and his wife, Emma, who struggled against her husband's chauvinism to define a life of her own. She does not provide, however, a convincing explanation for Bryant's single-minded pursuit of political success in Georgia, where, as a Yankee, his chances were so slight to begin with. Why did he hang on for so long, particularly after the Republican party had lost control of the state for good?

RICHARD H. ABBOTT
Eastern Michigan University

PAUL ANDREW HUTTON, editor. *Soldiers West: Biographies from the Military Frontier.* Foreword by ROBERT M. UTLEY. Lincoln: University of Nebraska Press. 1987. Pp. xiii, 276. Cloth $19.95, paper $9.95.

Paul Andrew Hutton provides a collection of essays on the careers of fourteen key military leaders in the trans-Mississippi West. Three of the army officers served in the antebellum period, one during the Civil War, and ten in the postwar era. The authors are experienced scholars, and the essays are well documented in primary sources and secondary works and illustrated with a portrait of each subject. The four maps, previously published, are adequate except for the lack of coverage of the period before 1850.

Hutton succeeds in representing the wide diversity of roles of officers on the frontier; they contributed to westward expansion in many ways other than combat. The book enables the reader to compare the attitudes of different officers to the Indians. George Crook was an innovative strategist and hard-line negotiator, but, like several others, in peacetime he became a defender of Indian rights and a champion of their welfare. Early in his career John G. Bourke believed that "the only good Indian was a dead Indian and that the only use to make of him was that of a fertilizer" (p. 141). He mounted the ears of a dead Apache warrior in a frame and used the scalp for a lamp pad. When a comrade almost fainted at the sight, Bourke had the ghastly trophies buried. Eventually, after close contact with Apache scouts, he developed admiration for the Indians and their culture, went on to conduct scholarly research, and made a significant contribution to anthropology. Several of the essays are original and valuable introductions to the careers considered, and the notes constitute a useful bibliography.

The format of the book, however, causes inherent difficulties. Most of the authors have published biographies or monographs on their assigned subjects, and it was a great challenge to discuss the life of such a prominent leader as Philip H. Sheridan in a chapter-length discourse. The essay on Nelson A. Miles by Robert M. Utley is well written, and other contributions are interesting, but most leave the reader struggling through brief factual summaries that only survey the highlights. Yet, the most disconcerting disadvantage is the repetition. In separate essays concepts such as the reservation policy are introduced and reintroduced, and the Great Sioux War and other campaigns are described over and over. The book is useful as an introduction to the careers of these individuals and provides a beginning for the comparative study of their activities and attitudes, but the serious student must proceed to more in-depth works.

JAMES A. RAMAGE
Northern Kentucky University

LOUISE CARROLL WADE. *Chicago's Pride: The Stockyards, Packingtown, and Environs in the Nineteenth Century.* Urbana: University of Illinois Press. 1987. Pp. xvi, 423. $32.50.

American social history has proceeded through three stages. Its original practitioners, concerned mainly with discovery, were content to describe the facts. Devotees of the new social history that appeared in the 1960s considered it the most exciting development in the profession because it substituted objective analytic categories, often quantitative, for impressionistic interpretation or, even worse, mere description. Today's social historians, aware of the limitations of the aging new social history, attempt to examine *mentalités* to discern how their subjects experienced their lives. Louise Carroll Wade's book takes us back to stage one. Reminiscent of (as well as indebted to) Bessie Louise Pierce's three-volume *History of Chicago* (1937–57), it is a work of indefatigable research, exhaustive detail, and no conceptual framework.

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Wade's study is a compendium of unrefined data, an encyclopedic rather than analytic piece of scholarship. Its virtue is as a reference work providing massive and comprehensive information on every aspect of the meat packing industry and its residential community. Technology, labor relations, sanitation problems, political issues, the Town of Lake, and the impact of meatpacking on Chicago and its railroads are painstakingly described. Business organization (for example, the structure of the chief firms) is less adequately examined, and this is a major weakness of the book.

But the chief flaw of *Chicago's Pride* is its author's rigorous adherence to descriptive detail. Wade tells us that Chicago pulled ahead of St. Louis because of greater railroad mileage, thus ignoring other factors and assuming that railroad construction is an independent variable. The author argues that the packers were generous public benefactors by presenting some of their contributions but does not indicate what portion of their wealth went for altruistic expenditure or what motives impelled their charity. She implies that the workers had no economic reasons for striking by noting that average wages and consistent employment in meat packing were higher than in other industries. But Wade does not investigate, in an era of low wages, whether these payments were adequate for a decent standard of living. She discusses some aspects of relations between ethnic minorities, particularly the Irish and Germans, but does not examine interactions between these groups in the workplace. Here, she might have profited from the work of James Barrett, who explored this issue on both the descriptive and analytical levels.

The weakness of Wade's book is encapsulated in this statement: "Historical evidence, not models or theories, determined the conceptual framework: This is an account of what *did* happen, not what *ought* to have happened" (p. xii). A mature scholar should know that models and theories are usually not counterfactual. As a rule, they attempt to clarify and interpret what actually occurred. Too many historians cavalierly dismiss models and theories, and when they do so they frequently write useful but not distinguished monographs.

FREDERIC COPLE JAHER
University of Illinois

DAVID SCHUYLER. *The New Urban Landscape: The Redefinition of City Form in Nineteenth-Century America.* (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1986. Pp. xiv, 237. $29.50.

In the latter half of the nineteenth century, a civic and cultural elite sought to implant a more naturalistic urban landscape in America's gridiron-constrained cities. Motivated by a commitment to the nation's "republican destiny" (p. 7), that elite envisaged a "new and civilized urban landscape, a more openly built urban environment that combined the morality they attributed to nature and the enriching and refining influence of the nascent cultural institutions that were making the city more and more attractive as a place of residence" (p. 2). So argues David Schuyler in this well-researched and gracefully written history of a largely unsuccessful effort at urban reform.

Schuyler's book is essentially an intellectual history focused on the work of Andrew Jackson Downing, Calvert Vaux, Charles Eliot, and, especially, Frederick Law Olmsted. Divided into three parts, it moves in part 1 from the "flawed visions" of early New York and Washington to the construction of a more pastoral and naturalistic urban ideal as exemplified in the rural cemeteries of the 1830s. Part 2 is concerned with the evolution of the park idea and the construction of parks (notably New York's Central Park) and parkways. And part 3 tells of how suburban communities were conceived as places for a domesticated landscape that would balance residential and commercial needs.

The above topics are, of course, familiar. What distinguishes this book is, first, its abundance of quotation—some may even find a surfeit of quotation. Second, the book is distinguished by the goal attributed to Olmsted *et al.*, namely, maintaining republicanism, the apparent source of Schuyler's avowedly positive outlook on his subjects (pp. 6–7).

One wishes, however, that Schuyler had better explained the problem of maintaining republicanism and its relationship to the city's physical form. The suspicion lurks that the problem, more completely stated, was one of maintaining democratic rule in the context of a capitalist society with its attendant inequalities, and vice versa. Schuyler cannot discern this, however, since in writing a self-contained intellectual history he relies on his subjects' own explanations to understand the meaning of their words and deeds. A greater attention to economic forces might also help explain the resilience of the gridiron form, about which he says surprisingly little beyond reciting Sam Bass Warner's culturally deterministic argument (*The Private City* [1968]) about "privatistic values" impeding reform efforts (p. 166). Finally, the absence of a thematic conclusion leaves the book somewhat truncated.

RICHARD E. FOGLESONG
Rollins College

W. BRUCE FYE. *The Development of American Physiology: Scientific Method in the Nineteenth Century.* (Henry E. Sigerest Series in the History of Medicine.) Balti-

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more: Johns Hopkins University Press. 1987. Pp. xi, 308. $35.00.

In his famous report on medical education, Abraham Flexner argued that the science of physiology was "the central discipline of the medical school." With this phrase Flexner expressed a consensus among medical educators that students of medicine required direct, "hands-on" experience in the physiological laboratory in order to prepare them for the practice of medicine. The reform of medical education in the United States in the last decades of the nineteenth century is an often told tale. What is new in the present book is a focus on "the central discipline."

W. Bruce Fye examines the careers of four prominent American physiologists: John Call Dalton, Jr., (1825–89), S. Weir Mitchell (1829–1914), Henry Pickering Bowditch (1840–1911), and H. Newell Martin (1848–96). Dalton and Mitchell received their training in physiology from Claude Bernard, the great French physiologist. Dalton eventually secured a chair at the College of Physicians and Surgeons in New York and became one of America's most vocal advocates for vivisection, both in classwork and research. Mitchell, despite a promising early career in physiology, failed to obtain a chair at the University of Pennsylvania or Jefferson Medical College in the 1860s. Mitchell thus was forced to practice clinical medicine. Bowditch, after training with Carl Ludwig in Leipzig, returned to Harvard where he set up an experimental laboratory that became a focal point for medical reform at Harvard. Martin, an Irishman who trained in Britain under William Sharpey and Michael Foster, became the first professor of biology at Johns Hopkins and leader and advocate for experimental research in medical education. Neither Bowditch nor Martin practiced medicine.

Fye argues that the two keys to reform in medical education in the United States were acceptance of the principles that faculty members not practice medicine and that the primary obligation of the full-time faculty member was experimental research. Fye sees these two goals first established in physiology, most clearly in the careers of Bowditch at Harvard and, especially, Martin at Johns Hopkins. The ideal of a full-time research faculty was accepted in Flexner's report and widely institutionalized through the Rockefeller-sponsored General Education Board. Research, not teaching or practice, became the norm in the medical faculties.

Fye has produced a well-argued case. There are a few areas that beg for more detail. Foreign training was a motivating factor for the Americans, yet we learn little about their experiences abroad. Although this is not a book that delves into the internal history of physiology, I would like to know more about how the research carried out by Fye's four protagonists fits into the broader picture of nineteenth-century physiology. I am surprised that Fye, a clinician, does not comment more about the fundamental decision of removing medical teachers from direct patient care responsibilities. And it is surprising to me that American medical educators embraced the promise of research physiology in an era when the practical benefits of experimental physiology could seldom be seen by either practitioners or patients. This book deserves a wide readership.

PETER C. ENGLISH
Duke University Medical Center

CINDY SONDIK ARON. *Ladies and Gentlemen of the Civil Service: Middle-Class Workers in Victorian America.* New York: Oxford University Press. 1987. Pp. viii, 234. $29.95.

The first female American white-collar workers were hired to clip and count currency for the Bureau of Printing and Engraving during the Civil War. They were soon joined in government offices by thousands more middle-class women and men as the federal bureaucracy expanded. Cindy Sondik Aron examines how this new labor force defined issues of class, gender, and work between 1860 and 1900. By doing so, she chronicles the birth of the modern white-collar bureaucracy in the United States.

The book is organized into three parts, exploring identities of government clerks, the evolution of the federal bureaucracy, and the emergence of a sexually integrated, middle-class workplace. The author skillfully weaves individual histories of workers into a narrative that relies on material drawn from application files kept by the Interior and Treasury Departments. Both men and women clerks, Aron suggests, perceived their middle-class status to be imperiled by federal employment. Like other scholars, Aron finds that women's social status was jeopardized by economic independence. The class status of men, however, was threatened for the opposite reason: salaried work in the fledgling bureaucracy presaged for them a loss of autonomy—a virtue at that time associated with ideal male employment.

Civil service, however, was to be defined as a middle-class occupation and thus helped reshape the values associated with employment for both men and women. Aron further argues that nineteenth-century civil service innovations—including salaried work, hierarchical management, and sexual integration—anticipated later employment practices in the private sector. A pioneering work culture flourished as middle-class men and women came together as colleagues for the first time.

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As Aron notes, her generalizations need testing against evidence from histories of private corporations. Her emphasis on sexual integration and work diversification (resulting from the absence of a centralized employment policy rather than enlightened administration) seems to minimize equally compelling evidence for the early roots of systematic sexual discrimination in the white-collar workplace. Minor quibbles include the thin index, lack of a full bibliography, and absence of a guide to the tables and photographs.

These caveats do not diminish the value of this excellent book. It is a fascinating account of the men and women who were part of the transition in middle-class labor in the second half of the nineteenth century. It will contribute to the current discourse on the history of labor segmentation and segregation and is an important addition to the literature in American labor, social, and women's history.

LYNN Y. WEINER
Roosevelt University

CYNTHIA GRIFFIN WOLFF. *Emily Dickinson*. New York: Alfred A. Knopf. 1986. Pp. xii, 641. $25.00.

Emily Dickinson's poetry is both work of unique beauty and a means of understanding the values and sensibilities of nineteenth-century New England. Cynthia Griffin Wolff's detailed, intense, and well-documented biography accomplishes two similar tasks: on the one hand it deals with a person of rare genius; on the other it illumines the lives of many American women and the context of their thoughts.

The central themes of the poet's life, and, therefore, as the author believes, of her poetry, are "the determination to confront the full barrenness of human fate" (p. 148) and the view that "all of human experience" is "essentially wounded by the malignity of a disappearing God" (p. 385). Dickinson's preoccuation with the trauma of separation is traced largely to a passive mother who loved but could not communicate and an active father, rarely at home, preoccupied with dynastic ambitions fulfilled only by his son and possessed of an intense sense of personal isolation. However, because she was a poet, as well as a woman living in a restricted and constricted situation, she spoke of a tragedy central to human existence, rather than a litany of personal grievances. It is this mixture of passion and intellect that is analyzed at length in the poetry. The gentle recluse of myth is replaced with the uncompromising poet of savage, mysterious verse, whose "arrogant, revisionary image of Heaven" (p. 341) comes close to Milton's version of hell. This bitterness is the result of her conclusion that "this God of absolute cold" had "concocted a cosmos that functions primarily as an experiment in human anguish" (p. 321). Sunrise brings no pretty fancy of "rosy-fingered dawn" but is rather "the opening volley of God's assault upon mankind." The reader of this woman's work is left to wonder how much similar bitterness was the general lot of her contemporaries who lacked her ability to turn impotence and rage into words.

Wolff's analysis of the conversion experience as it applied particularly to nineteenth-century women is acute, and she has also explored the way in which biblical and secular imagery found their way into Dickinson's verse. It is possible to wonder about some of her conclusions. For example, although Dickinson's refusal of faith is seen as a commitment to poetry and an impressive choice of "the examined life" over the "abject submission" that was "a prerequisite for faith" (p. 85), other gifted persons came to different conclusions. Wolff's knowledge of and enthusiasm for her subject (excellent things in a biography) occasionally make Dickinson seem to be more contemporary than she probably was. The discussion of her various voices—the child, the wife, the speaker on unspeakable subjects—is very convincing, and the analysis of Dickinson's own loves equally so. Wolff points out that Dickinson's draft letters to Judge Otis Phillips Lord are striking in their "unmistakable element of sexual passion" (p. 402). This portrait of an intense, complex woman should, but probably will not, end the bizarre or mealy-mouthed or vacuous interpretations of Dickinson's life and poetry that have abounded. Wolff is particularly faithful to her sources and comments on what is not known, and probably cannot be known, because of missing documentation.

BARBARA WELTER
Hunter College and Graduate Center
City University of New York

WILLIAM R. HUTCHISON. *Errand to the World: American Protestant Thought and Foreign Missions*. Chicago: University of Chicago Press. 1987. Pp. xii, 227. $24.95.

This small volume represents a serious effort to make up for the relative neglect of the subject of foreign missions by students of American religious life and institutions. William R. Hutchison, whose interest in the topic arose from participating in one of John Fairbank's projects on American–East Asian relations, has produced a solid account of the development of American Protestant missionary ideology from the Puritan fathers—with a glance at Roman Catholic predecessors—to the present. Drawing on a family missionary background as well as in-depth

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study of the missionary literature, he is able to balance sympathetic understanding with critical analysis. Much of the book is devoted to a discussion of the writings of missionary theorists and home-base promoters such as Cotton Mather, Jonathan Edwards, Rufus Anderson, A. T. Pierson, James Dennis, John Mott, and Robert Speer, with only a few references to the ideas of practicing missionaries in the field. In a chapter with the provocative title, "A Moral Equivalent for Imperialism," derived from William James, the author wrestles with the thorny question of the convergence of Christian world mission with American and European expansionism, but without conclusive answers.

A major theme running throughout the book is the familiar, never-resolved debate over the issue of civilizing versus evangelizing strategies. Hutchison does not find significant variations in missionary ideology on a confessional or denominational basis, but he occasionally notes differences between Anglo-American and Continental European approaches to foreign missions and calls special attention to some European critiques of American "activism." In general the missionary movement seems to have acted in its heyday as a unifying force among Protestants, joining men and women of disparate views—premillennialists and postmillennialists, theological conservatives and liberals, advocates of personal redemption and the Social Gospel—in a common cause. But this consensus broke down in the 1920s and later, as the author relates in the last two chapters. The Report of the Laymen's Inquiry on Missions of 1932 featuring the revisionist philosophy of foreign missions of its chairman, William E. Hocking, fueled a controversy that drew lines of division between liberals, moderates, and conservatives. While the mainline Protestant churches witnessed a decline in missionary zeal, fundamentalist and other conservative foreign mission agencies multiplied. Despite the resulting stand-off between "ecumenicals" and "evangelicals," the author suggests the possibility of the emergence of a new international outlook combining Christian witness with acceptance of religious and cultural pluralism. This book furnishes an excellent guide to future studies of the American Protestant missionary mind.

CLIFTON J. PHILLIPS
DePauw University

DOUGLAS FRANK. *Less Than Conquerors: How Evangelicals Entered the Twentieth Century.* Grand Rapids, Mich.: William B. Eerdmans. 1986. Pp. x, 310. $14.95.

This is a book about the heart of American evangelicalism from 1850 to 1920—that group of "white, northern, middle-class Christians who had been strongly influenced by Calvinistic theology—in other words, Baptists and Presbyterians" (p. viii). Like much of the best recent writing about American religious history, it is the work of an evangelical scholar.

More than most recent offerings, however, Douglas Frank's book is an exercise in moral instruction. It is roughly half history and half sermon, with a smattering of personal confession. The sermon is a rather harsh condemnation of his fellow evangelicals: "We are the Pharisees of our time" (p. 229). The problem with evangelicals, surmises Frank, is that they have been power hungry, self-satisfied, and spiritually arrogant. The author was surprised that some of his evangelical readers thought he had been a bit harsh; I will be surprised if he is not labeled a spiritual elitist of the rankest sort himself. But who will win the battle of proof texting is not our primary concern here.

Historically, Frank chronicles the story of three themes within evangelicalism: the rise of dispensational premillennialism, the spread of the doctrine of perfectionism, and the revivalism of Billy Sunday. In some vigorously written passages, he shows how each of these themes meshed with the growing perception of evangelicals that they were losing control of American society.

Dispensational premillennialism replaced the early nineteenth-century postmillennialism partly because it squared with the growing gloom in the evangelical community. On the other hand, dispensationalism was not simply a defeatist view of history; evangelicals turned biblical prophecy into a triumphant assertion that the church would be victorious in spite of appearances. The "Victorious Life" movement of the late nineteenth century provided a similar assurance to evangelicals who were losing control. They laid claim to a life of perfection, soothing the anxieties created by social change and aligning them in some ways with the escapism of Christian Science and the mind-cure sects. Finally, the revivalism of Billy Sunday held out the possibility of regaining power by reforming the nation. Sunday's call for manliness and character and the hero cult that developed around him were once again symptoms of evangelical self-confidence in the midst of tribulation.

This is a very good book—if one can stand the moralizing. The author is more familiar with the sources at some points in his story than at others. He is particularly at home in dealing with the writing of Charles B. Trumbull and with Billy Sunday. In other places he leans heavily on the works of others. But the value of Frank's book is its interpretations. They are based on broad interdisciplinary reading

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and a profound personal understanding of evangelicalism.

DAVID EDWIN HARRELL, JR.
University of Alabama, Birmingham

ROBERT T. HANDY. *A History of Union Theological Seminary in New York.* New York: Columbia University Press. 1987. Pp. xiv, 378. $30.00.

Union Theological Seminary in New York was founded by nine Presbyterians—five laymen and four ministers—in 1836. To celebrate its one hundred fifty years—years during which the seminary grew into a major, world-renowned, interdenominational institution—Robert T. Handy has written this excellent history. The story he tells is an important one, too, because Union has mirrored and influenced the course of American religious history perhaps more than any other seminary.

Handy's book gives due attention to the seminary's Presbyterian roots and to the stormy relationship with that denomination's right wing that led the school to make its way independently after 1892. Actually, Handy gives greatest attention to the seminary's history from that point on, which makes good sense because two earlier, serviceable histories bring the story up into the twentieth century and it is then that the school entered the era of its greatest influence. Handy provides a wide-ranging assessment of Union's growth, balancing administrative and financial matters with curriculum and student life. But it is the faculty that gets the spotlight. That, too, is as it should be, for Union's faculty has been its real glory—Henry Boynton Smith, Philip Schaff, and Charles Briggs in the nineteenth century; A. C. McGiffert, Henry Sloane Coffin, Henry P. Van Deusen, John C. Bennett, and, of course, Paul Tillich and Reinhold Niebuhr in the twentieth. The faculty now includes and benefits from individuals such as James Cone, Phyllis Trible, and Kosuke Koyama, and that tells a great deal about the increasing inclusiveness not only of Union's hiring policies but also of contemporary theology in general.

Handy writes with the cautious restraint and good sense that we have come to associate with his work. He generally lets the participants speak for themselves and does little editorializing, although in some instances, such as when Union summarily fired a professor for his opposition to American involvement in World War I, Handy's displeasure is readily apparent. But mostly this is a record of successes. If there is a theme that holds Handy's story together, it is continuity. The founders had a vision of a seminary that balanced piety and learning, that prided itself on the advantages of an urban location, that took the high road above denominational disputes, that valued a social conscience, and that sought reconciliation and moderation in all things at all times. Handy finds that those ideals from the 1830s have remained remarkably constant from then until now. (Although one has to wonder how much "moderation" truly characterized the 1960s at Union or indeed whether the founders could comprehend, in any sense at all, sit-ins by radical students or the arrest of President Bennett while he was in Washington, D.C., protesting the Vietnam War.)

This is, unquestionably, a good book, and, whereas many histories of individual education institutions appeal only to readers who have personal connections to the school, this one deserves, and will likely receive, a much wider audience.

CHARLES D. CASHDOLLAR
Indiana University of Pennsylvania

L. H. GANN and PETER J. DUIGNAN. *The Hispanics in the United States: A History.* Boulder, Colo.: Westview and Hoover Institution on War, Revolution, and Peace, Stanford, Calif. 1986. Pp. xv, 392. $28.85.

Hispanics are defined by the U.S. Census Bureau as Spanish-speaking people of Spanish or Latin American origin. This grouping, however, exists primarily for demographic convenience and has little bearing on historical or political reality. As this book makes clear, there are many more differences between the various Hispanic groups within the United States than there are similarities. Differences of citizenship status, race, socioeconomic condition, language, elements of popular culture, geographic concentration, and historical background make for a tremendous diversity among groups constituting the Hispanic minority: Mexican-Americans (Chicanos), Puerto Ricans, Cubans, Central Americans, and other Latin Americans. Despite this diversity, L. H. Gann and Peter J. Duignan want to argue that they are all unified by common aspirations to participate in the American dream of material success. Gann and Duignan propose that Hispanics are essentially like many of the European immigrant groups before them.

This is an unusual book in several respects. It is unabashedly patriotic in its assumptions and argumentation. In this perspective the authors claim to be closer to the views of the immigrants than liberal or ethnic interpreters: the authors are highly critical of ethnic radical rhetoric. They are optimistic about the future economic and social success of Latinos within the United States.

This book is not primarily an apology for the American dream. The authors have mined a vast and complicated body of academic literature to

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analyze very real economic and social problems. As the facts are presented, the authors are very candid about their own opinions.

The subtitle of the book is misleading. Only about one-half of the book is a survey history of the major Hispanic groups in the United States; the history of Puerto Rico, the Mexican and Cuban revolutions, and the Chicanos since 1848 is condensed to about one hundred pages. Inevitably, topics such as the history of labor organizing and the violent confrontations between Mexicans and Anglo-Saxons in Texas and California are given scant mention.

About two-thirds of the book is devoted to an analysis of contemporary issues. The discussion of immigration and the rise of the new Hispanic cultures is impressive in its scope. The least satisfactory chapters are those devoted to the Catholic church, drug addiction, and crime. Here the authors have failed to differentiate between Hispanic groups and seem to wander from any coherent thesis.

Although experts in the many fields covered will probably be disappointed, the factual basis of this book remains impressive. As an introduction to the history and social problems confronting the Latino populations in the United States, it is provocative reading.

RICHARD GRISWOLD DEL CASTILLO
San Diego State University

SHIH-SHAN HENRY TSAI. *The Chinese Experience in America.* (Minorities in Modern America.) Bloomington: Indiana University Press. 1986. Pp. xv, 223. Cloth $29.95, paper $9.95.

The central objective of this book is to familiarize readers with the evolutionary patterns of three groups: the China-oriented sojourners of the nineteenth century, the American-born Chinese, and the highly educated student immigrants of the post–World War II period. Shih-shan Henry Tsai examines the successes and failures of these groups within a historical context and a framework based on theories of acculturation and assimilation. He characterizes the Chinese-American experience as one fraught with cultural conflicts, political alienation, and many injustices. The author emphasizes, nevertheless, that American views of the Chinese have changed from highly unfavorable in the past to highly favorable in the present.

The first section highlights early immigration and the development of Chinese communities and organizations, together with reactions to the later American exclusion of the Chinese. Accounts of the difficulties that faced the sojourners and the American-born Chinese at the end of the nineteenth century and up to World War II present neglected dimensions of the connections between Chinese-American life and major historical events in China. The importance of understanding the post–World War II transitions is emphasized through discussion of the entry of several waves of highly educated student immigrants, which include the "stranded students" admitted to the United States when the Communists gained control of the Chinese mainland and the new immigrants who have entered the country since 1965. The author concludes that the Chinese in the United States can no longer be studied as a homogeneous group, given generational and occupational differences and their varied places of origin.

Some scholars may disagree with Tsai's position that the difficult experiences of the early Chinese sojourners were related, in part, to their own resistance to acculturation. He proposes that adherence to Chinese heritage and the maintenance of traditional systems of governance within Chinatown enclaves retarded the acculturation of the first and second generations. Although this proposition merits attention, the concepts of acculturation and assimilation, which are basic to the author's model for the evolution of Chinese-American society, need elaboration in light of the varied uses and meanings of these concepts.

Overall, however, this work advances our understanding of the social organization of Chinese Americans, particularly the changing character of traditional associations and social movements. This book and the author's earlier work, *China and the Overseas Chinese in the United States, 1868–1911* (1983), which is a major source for the present study, should become basic references on the subject. It is a welcome contribution to the general history of the Chinese in the United States, and it should serve as an introduction to students of American ethnic history.

LUCY M. COHEN
Catholic University of America

GARY R. MORMINO and GEORGE E. POZZETTA. *The Immigrant World of Ybor City: Italians and Their Latin Neighbors in Tampa, 1885–1985.* (Statue of Liberty–Ellis Island Centennial Series.) Urbana: University of Illinois Press. 1987. Pp. xiii, 368. $24.95.

Historians and other educated individuals interested in the degree of sophistication that immigration history has achieved in the last thirty years will have to read this book. Gary R. Mormino and George E. Pozzetta have successfully blended labor and immigration history in the analysis of the establishment and growth of the multi-immigrant community in Ybor City, Florida, a city envisioned by its founder, Vicente Martinez Ybor, as a company town to support the cigar-manufacturing industry. Span-

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iards, Cubans, and Italians were attracted, each group for its own reasons, to Ybor City; each group struggled to create a space of its own in the local industry; each group eventually put down roots in Ybor City and established separate institutions, which eventually merged and provided the foundation of a cohesive Latin community. The achievement of that Latin community occurred through a difficult process, because of both internal and external problems. Internally, immigrant groups had to learn the difficult lesson of political compromise among competing immigrant and ethnic interests; externally, all immigrants, regardless of origin, had to fight to achieve higher wages and better working conditions from cigar manufacturers. Anarchism, socialism, and communism thrived in Ybor City in the first two decades of the twentieth century, a testimonial to an America more flexible and less structured than the corporate America of today. The authors document the rise of militant labor unions, radical clubs, and consumer cooperatives in Ybor City—phenomena usually associated with the labor movement in the Northeast. Today Ybor City is more a bittersweet memory than a reality; urban renewal has razed most of the immigrant housing, and Spaniards, Cubans, and Italians, who left cigar manufacturing for a more diversified economy in the 1930s, were displaced. The authors have successfully rescued a past about to disappear from memory as fast as the physical environment of immigrant Ybor City has recently vanished.

From this brief summation, no one should conclude that this is, at best, a successful book in local history. On the contrary, contextual references to the national scene reveal both the uniqueness of the Ybor City immigrant experience and its place within the national scene. The intelligent use of oral history is another major asset of this work; Ybor City's past is resurrected through both written documents and the discriminating use of the immigrants' memories. Equally commendable is the authors' skill at pointing out the ironies of immigrant radicalism: the immigrants' commitment to radical ideologies was almost as deep as a religious faith; the reality of their lives was influenced more profoundly by communal cooperation than by radical ideologies. Perhaps, although attracted by the radical ideologies emerging from industrialization and modernization, immigrants remained, at heart, the inhabitants of rural societies characterized more by communal cooperation than by class consciousness and social conflict.

The major contribution of this community study is the analysis of the core dynamics that gave life in Ybor City cohesiveness and lasting validity. The authors' conclusion is insightful, though not totally new. Outside agencies, such as churches, were rejected; Old World traditions helped, but only to a degree, since the Ybor City environment was so different from the past; American society was by and large absent too, except at election time. The immigrants' human and humane resourcefulness, expressed in community life unfolding in saloons, groceries, club halls, union rooms, and debating forums, created social belonging and provided the immigrants' lives with lasting meaning. Immigration historians have long wondered whether to emphasize Old World traditions or the American environment in the study of the immigrants' experience. Mormino and Pozzetta, among others, have raised a new question. Perhaps the key to the understanding of the immigrant experience lies where most historical processes have to be finally traced: in the resourcefulness of the minds and hearts of immigrants themselves. Will we, or our children, ever be allowed or forced to draw on our personal resourcefulness with the same intensity as our immigrant ancestors? Perhaps our corporate society has made that resourcefulness impossible both as an attitude and as a way of thinking.

DINO CINEL
Tulane University

MELVYN DUBOFSKY and WARREN VAN TINE, editors. *Labor Leaders in American History*. (The Working Class in American History.) Urbana: University of Illinois Press. 1987. Pp. xvi, 396. Cloth $34.95, paper $14.95.

This book will undoubtedly be hailed by many labor historians. But I am not able to join the enthusiasts. On the contrary, I view the work as a shameful example of sophisticated anti-Communism. Rather than indulge in the simplistic anti-Communism of many historians of the 1950s, the editors and authors of the present volume have substituted the practice of simply ignoring the role of Communists. One would never know from reading this book that organizations such as the Trade Union Educational League or the Trade Union Unity League even existed, let alone that they played an important role in the labor movement of the 1920s and early 1930s. William Z. Foster, head of both of these organizations as well as leader of the Great Steel Strike of 1919 and general secretary of the Communist party of the United States, rates one mention. This is in Joseph R. Conlin's essay on William D. "Big Bill" Haywood, where Foster is merely listed among the propounders of "the syndicalist ideology" (p. 117). Elizabeth Gurley Flynn, the "Rebel Girl" of the Industrial Workers of the World, active and militant leader of so many Wobbly strikes, including its Lawrence, Massachusetts, strike of 1912, also rates one single mention. Again this is by Conlin in his essay on Haywood where she is described in a contemptible comment as "later a Communist party

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workhorse" (p. 129). Harry Bridges does not even merit a single mention, let alone a deserved biographical essay. Ben Gold is not listed in the index, but the Fur and Leather Workers' Union, which he headed for so many years, is listed in a way designed obviously to avoid mentioning the dreaded name of a man who was a member of the Central Committee of the Communist party as well as a brilliant trade union leader. In the essay on Philip Murray by Ronald W. Schatz we read the following: "An indication of Murray's personal feelings in this area [anti-Communism] was his refusal to let his wife wear a fur coat that she had been given by the head of the Fur and Leather Workers Union, the reddest of the CIO's affiliates" (p. 251). Of course, given the outlook of the book, Schatz has no criticism of Murray's "personal feelings." Similarly, Robert H. Zieger, in his essay on George Meany, is critical of nearly every aspect of Meany's labor outlook except his paranoid anti-Communism and his support of reactionary regimes in Latin America, Asia, and Africa.

"Here are the life stories of the men and women who have led the labor movement in America from recent times to the present," proudly asserts the book jacket. The reader will soon discover that it should read: "Here are some of the life stories, etc. All men and women who were Communists or militant left-wingers are not included, no matter how important a role they played in the American labor movement."

No one expects a biographical essay to be a full-length biography. But too often there is a tendency in this volume to omit essential aspects of a labor leader's career—especially if they relate to any association with the Left. David Montgomery (whose treatment of William H. Sylvis's views on the Negro question is superficial and one in which the word "racism" never appears) says nothing of Sylvis's internationalism as president of the National Labor Union. He does not mention or quote his magnificent letter to the General Council of the First International in response to the council's appeal, written by Karl Marx, to prevent a war between the United States and England over the *Alabama* claims. Nick Salvatore, in his essay on Eugene V. Debs, omits any reference to the facts that the American Railway Union barred blacks from membership and that Debs, who opposed this action, attributed the loss of the Pullman strike (during a speech in Harlem in 1923) to this racist action. Salvatore does not even mention the Debsian view on the Negro question, which had an unfortunate influence on the Socialist party's backward approach to this important issue. Salvatore, too, says nothing about Debs's enthusiastic support of the Bolshevik revolution while in prison and his endorsement of the Trade Union Educational League.

In his essay on Sidney Hillman, Steven Fraser does not even mention the fact that Hillman returned from a visit to Soviet Russia in 1921 filled with enthusiasm. He found "order preserved even in economic ruin," efficient factories producing clothing fit to wear in the United States, and an incomparable appreciation by labor of its duties in society. Fraser does not mention the role in behalf of labor solidarity played by the Amalgamated Clothing Workers, under Hillman's leadership, in the formation of the Amalgamated Textile Workers of America and in the great Lawrence strike of 1919.

In their essay on John L. Lewis, Melvin Dubofsky and Warren Van Tine write: "One must recognize the role of a militant minority among the rank and file in the automobile industry." But who was this "militant minority"? To answer this question would require uttering the dreaded word, "Communists." Hence the reader is left in the dark.

With some of the same limitations already mentioned, I found the essays on William Green by Craig Phelan, Rose Schneiderman by Alice Kessler-Harris, and Jimmy Hoffa by Estelle James interesting and useful. At least they present information on these leaders that is not easily available.

But one omission that applies to these as well as the other essays is the absence of reference notes. The only guides to sources are bibliographical notes, which are too often inadequate. This enables the authors to make sweeping assertions without furnishing adequate evidence. I could cite many examples, but typical is Richard Oestreicher's assertion in his essay on Terence V. Powderly that "Republican ideology" dominated smaller industrial communities in the United States during the 1880s (pp. 34–35). It would be interesting to learn what evidence there is for this declaration.

I left this book convinced that nearly all of the labor leaders discussed remained in leadership too long and that removing them from office when their usefulness, such as it was, was over proved to be impossible. But only one author, Phelan in his essay on Green, even bothers to deal with this issue.

This book is part of the series "The Working Class in American History" published by the University of Illinois Press. If the presentation in future volumes of this series is similar to that of the present volume, the outlook for the honest writing of American labor history is grim indeed.

PHILIP S. FONER,
EMERITUS
Lincoln University, Pennsylvania

DAVID GARTMAN. *Auto Slavery: The Labor Process in the American Auto Industry, 1897–1950.* (Class and Culture.) New Brunswick, N.J.: Rutgers University

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Press. 1986. Pp. xv, 348. Cloth $42.00, paper $20.00.

This book stands squarely in the tradition of historical sociology pioneered by Harry Braverman's *Labor and Monopoly Capital: The Degradation of Work in the Twentieth Century* (1974). It is a history of the way automobile industry managers reorganized the labor process in the first half of this century, not only to increase the efficiency of their factories but to enhance their control of those who labored within. In many ways this is a familiar story, but David Gartman's insightful reading of industry sources makes it an excellent account of the way in which the innovations pioneered at Ford spread to other firms in the industry. Gartman demonstrates that the technological and organizational revolution symbolized by the assembly line represented a decisive weapon managers deployed against the power and shop culture of the skilled workers who controlled the labor process in the industry's first few years. Thereafter, a series of technical and social innovations—mechanized material handling, automatic machine tools, the famous "five dollar day," job ladders, incentive pay schemes, welfare programs, and company unions—kept managers in control of a sometimes restive work force.

But the auto capitalists could never find a formula that worked perfectly. Whenever the industry went through one of its periodic booms, labor turnover soared, work discipline sagged, and unionization efforts revived. Moreover, the growing size of auto factories and the integrated character of production gave workers the social power necessary to build a successful union. The United Automobile Workers (UAW) succeeded in slowing the work pace, curbing the capriciousness of lower supervision, and reducing wage differentials, but Gartman concludes his book by endorsing the work of those labor historians who have suggested that even the militant UAW did little to shift essential power relations on the shop floor. By helping create a new series of bureaucratic management tools—including grievance handling and arbitration—the union found itself drawn into a system that displaced the shop floor power some workers had held in the 1930s and 1940s.

The most tenuous analytical element in Gartman's work is his effort to distinguish between what he calls "neutral production techniques" and those "biased" by the capitalist ownership of the means of production. Thus, he finds some aspects of assembly line production—the division of labor, progressive layout, and the development of precision tools—"nonrepressive" because they are essential simply for efficiency and mass production. But Gartman also tries to define a separate category of "repressive" labor control techniques, including close supervision, speed-up, and incentive pay schemes, which reflect the particularly harsh and exploitative class relations characteristic of the industry (pp. 7–8).

Although the effort to sustain this distinction animates much of Gartman's book, it seems an entirely mechanical effort to try to figure out which production techniques were designed to enhance quality or volume and which were simply designed to sweat more labor out of the workers. At one point Gartman himself seems to recognize that "most techniques are a complex combination of both" (p. 9), but his subsequent effort to define and label these distinctions proves both annoying and confusing to the reader. Recent historians of labor and technology have taught us that the culture and technique of a particular stage in the evolution of capitalist production are a seamless web in which managerial attitudes about work, family, profits, pay, and machinery mutually reinforce one another. The design of a particular machine represents the outcome of a complex process that so thoroughly incorporates a whole set of social and technical conflicts as to make distinctions between "repressive" and "nonrepressive" functions meaningless. In this sense auto labor was indeed like the system of slavery to which Gartman compares it.

NELSON LICHTENSTEIN
Catholic University of America

DANIEL M. CRANE and THOMAS A. BRESLIN. *An Ordinary Relationship: American Opposition to Republican Revolution in China*. Miami: University Presses of Florida. 1986. Pp. xxii, 225. $25.00.

This volume explores a neglected area of Sino-American relations, the response of the American government to the Chinese revolution of 1911 and to the turmoil following it. Daniel M. Crane and Thomas A. Breslin believe that too many scholars still perpetuate the myth of a special Sino-American relationship; the authors argue that William Howard Taft and Woodrow Wilson did not pursue benevolent policies toward China. Despite their idealistic rhetoric, they were not concerned with the growth of democracy in China and instead sought to advance the political and economic interests of the United States. The nation's governing elite, the authors argue, was deeply concerned with the maintenance of stability both at home and abroad: "Their conservatism has been rooted in the fear that either failure to find foreign markets or the emergence abroad of attractive alternatives to the American political and economic system could lead to domestic unrest and loss of their political dominance" (p. 1). The result is an antirevolutionary heritage extending from George Washington to Ronald Reagan, of which Taft and Wilson were a part. "The case of

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China in 1911 was no different from that of Haiti or Hungary or Mexico or Russia" (p. 167).

Given this New Left framework that the authors impose on the whole of American foreign policy, it is hardly surprising that they find a remarkable consistency in the responses of the American government under Taft and Wilson to revolution in China. Taft chose to cooperate with the great powers; Wilson believed that American ends could be achieved independent of them. But the authors contend that the differences between the two administrations were minor and that both longed for stability and centralized rule, taking a narrow, self-interested view of America's interests. They believed that "penetration of the China market would thus make the United States a greater economic power and, significantly, it would quell the economic, political, and religious unrest at home which so concerned the American leadership" (p. 175).

In an effort to advance this argument, the authors carefully examine early American reactions to the revolution of 1911, the involvement of the United States in the six-power consortium and its loan negotiations, the lack of sympathy of American leaders for Chinese efforts to achieve political and economic independence, and the acquiescence of the American government to the emergence of a dictatorship under Yuan Shikai. Although they include many interesting details on Sino-American relations during these years, they fail to create a convincing reinterpretation of American China policy under Taft and Wilson. They dismiss the idealistic rhetoric of Taft and Wilson as camouflage for their pursuit of material interests, reject the autonomy of the missionary impulse, and ignore divisions within the business community as well as the bureaucratic determinants of American policy. Their vision of a unified national leadership pursuing grand strategies abroad simplifies and distorts American foreign policy. They offer an old-fashioned New Left interpretation that long ago proved inadequate in explaining the complexities of America's response to foreign upheavals.

CHARLES E. NEU
Brown University

WILLIAM B. PROVINE. *Sewall Wright and Evolutionary Biology*. (Science and Its Conceptual Foundations.) Chicago: University of Chicago Press. 1986. Pp. xvi, 545.

Studies of the collaboration between Sewall Wright and Theodosius Dobzhansky first led William B. Provine to suspect that Wright had exerted a significant influence on the development of modern evolutionary biology. By the time he had completed work on this excellent biography, Provine was convinced that future historians and biologists would recognize Wright as the single most influential evolutionary theorist of the twentieth century. Born in 1889, the year Alfred Russel Wallace published *Darwinism* and Francis Galton published *Natural Inheritance*, Wright has been a participant in and witness to almost the entire history of genetics and its relationship to twentieth-century evolutionary biology.

Although Wright was apparently interested in natural history and evolution by the age of seven, it was a college biology course taught by Wilhelmine Marie Entemann that led him to decide on a career as a biologist. As a graduate student with William Ernest Castle at Harvard, Wright immersed himself in the two major themes of his scientific career, physiological genetics and evolutionary theory, and began his long-term work on guinea pig genetics. The concept of gene interaction that emerged from his studies of the inheritance of color patterns in the guinea pig was fundamental to all his later work on physiological genetics and population genetics. While working at the U.S. Department of Agriculture, Wright invented the method of path coefficients, a powerful tool that essentially revolutionized the quantitative analysis of systems of mating.

Although Wright's papers were widely read, Provine admits that many biologists found them unreadable and his quantitative arguments obscure. Confronting this disparity between the influence and the comprehension of Wright's papers, Provine notes that many read Dobzhansky instead of Wright, because his prose was more accessible. Perhaps in biology as in economic theory, as John Kenneth Galbraith has said, writings that are unintelligible are accorded more respect.

Provine demonstrates that the highly visible conflict between Wright and R. A. Fisher had a profound influence on evolutionary theory and field research. Wright and Fisher shared certain fundamental assumptions about the relationship of Mendelism to Darwinism, but their disagreement about the basic processes of evolution was irreconcilable. As the evolutionary synthesis moved toward a more thoroughly adaptationist stance in the 1940s and 1950s, Wright felt that biologists tended to misrepresent his theory of evolution as pure random drift. But, with the subsidence of the most extreme selectionist attitudes in the 1970s and 1980s and the widespread influence of his four-volume series *Evolution and the Genetics of Populations* (1968–78), Wright's ideas have once again stimulated theorists and field workers.

In writing this biography, Provine had the remarkable experience of reading all of his subject's correspondence and published papers, discussing them with Wright, and having him read drafts of each chapter. Despite failing vision, Wright pro-

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vided about one hundred pages of comments. Does doing history of science this way interfere with objectivity? Although there is no doubt that Provine admires Wright for the breadth and depth of his scientific contributions, the text makes it clear that biographer and subject had cordially "agreed to disagree." Provine is certainly justified in asserting that having Wright read his manuscript prevented many errors, but do we have to accept Provine's implied judgment that other biographies of scientists, and perhaps all biographies, are necessarily "crammed with reasonable-sounding falsehoods" (p. xiv)? If this is true, elderly scientists may soon be surrounded by historians of science, much as the "last primitives" have been surrounded by anthropologists. Given the fact that there are more major scientists alive today than have ever lived in the history of the world, historians of science, unlike other historians, need not concentrate on subjects already dead.

LOIS N. MAGNER
Purdue University

JOHN G. CLARK. *Energy and the Federal Government: Fossil Fuel Policies, 1900–1946*. Urbana: University of Illinois Press. 1987. Pp. xxiii, 511. $39.95.

In the past three decades economic historians have written about federal regulatory policy toward specific fuels during the twentieth century. John G. Clark has gone one step farther in synthesizing and summarizing this material with his own extensive research to create a comprehensive picture of federal policy toward anthracite and bituminous coal, natural gas, and oil from the turn of the century to the end of World War II. Clark's book is the first major comparative study dealing with the regulation of these fossil fuels and its impact on the United States.

Clark has organized his book so that it will be useful to either the generalist who is interested in a survey of this subject or the specialist who desires in-depth information about a particular fuel in a specific time period from 1900 to 1946. The first chapter, "The Fuel Industry in the Early Twentieth Century," surveys the competitive nature of the different fuels as oil, gas, and electricity challenged coal as the mainstay for American energy needs. During this period the market for fuel also changed dramatically as the United States underwent rapid urbanization, and the population became dependent on the automobile. The last chapter summarizes the changing nature of federal fuel policy from 1900 to 1946 and evaluates its importance. Clark shows that the federal government created its policy in a haphazard manner in response to various events such as wars, depression, strikes, and fuel shortages. While competing businesses in the private sector seeking government aid usually clustered into the two camps of free-market advocates and antitrust exponents, groups within the federal government fluctuated between these viewpoints and failed to create a clearly comprehensive policy for the welfare of the entire country. As a result, Clark shows that the policy was vague, unsystematic, and essentially purposeless.

Clark goes into great depth in chapters 2 through 14 to prove his points, bringing into focus major companies and policies at the local and state level as well as the total national picture. The chapters are arranged chronologically from the pre–World War I period to 1946 and are divided into key subtopics such as specific fuels, agencies, policies, and events. Clark's ability to compress an extensive body of knowledge into three hundred ninety pages and skillfully make broad, well-written generalizations is indeed impressive.

Without a doubt this book is a major contribution to economic history and the history of federal policy. It will be the definitive history of this subject that many libraries should have as part of their collection.

H. BENJAMIN POWELL
Bloomsburg University

ROBERT ALAN GOLDBERG. *Back to the Soil: The Jewish Farmers of Clarion, Utah, and Their World*. (Utah Centennial Series, number 2.) Foreword by CHARLES S. PETERSON. Salt Lake City: University of Utah Press. 1986. Pp. xxviii, 196. $19.95.

Some historical themes contain surprising potential for analyzing divergent movements. This is true of efforts by Jewish immigrants to become farmers in the era of America's "last" farm frontier. The subject requires evaluation of European conditions, ideological factors, as well as the socioeconomic context of America at the turn of the century. It is not a new formula, but Robert Alan Goldberg succeeds admirably in applying it to the unique circumstances of Utah. He includes succinct yet scholarly surveys of relevant research held together by diligent use of freshly discovered biographical sources. Thus, family records and interviews serve to trace the journey of individual Clarion settlers from tsarist oppression to urban sweatshops and, ultimately, to Utah farming. Consequently, the personal feelings of pioneers emerge within the context of eventful transformation.

Out of the pogroms and prevalent anti-Semitism, Jewish agrarianism developed as a self-liberating movement. It united the impoverished refugees from Eastern Europe, some imbued with utopian ideals, and their acculturated western coreligionists

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concerned with the newcomers' productive resettlement. Thus, over forty Jewish rural communities emerged in the United States between 1881 and 1910, along with organized agrarian development in Palestine, Argentina, and Canada.

But there was also tension between the settlers and their distant benefactors. In the case of Clarion, Goldberg notes, the directors of the Jewish Agricultural Society determined that requests for subsidy had come too late in the wake of some failed agrarian ventures. Moreover, philanthropists may have considered the Clarion settlers overly independent, even "left-wing" in their "collective" approaches (p. 99).

In practice, both the members and leadership of the "back to the land" movement emerged from the Yiddish-speaking immigrant populace, including Benjamin Brown, who advocated western settlement to minimize the temptations of the East. Years of recruitment and sale of participatory shares led to an association that purchased over eight thousand acres from the State of Utah in 1911. It was one hundred fifty miles from the Salt Lake City markets, but eighty-one first families soon left the squalor of New York and Philadelphia to confront the challenges of dry farming. By the 1920s most had to admit defeat.

Goldberg recounts their story in lively fashion, supplementing the narrative with relevant quantitative information. He concentrates on people rather than organizational matters. And Charles S. Peterson's foreword provides useful background on the unmet hopes of countless other farmers attracted to arid lands, including clues to Clarion's demise. Altogether, this is a balanced and attractive model for regional ethnic-economic history.

JOSEPH BRANDES
William Paterson College

PETER GOTTLIEB. *Making Their Own Way: Southern Blacks' Migration to Pittsburgh, 1916–1930.* (Blacks in the New World.) Urbana: University of Illinois Press. 1987. Pp. xiii, 250. $26.95.

Peter Gottlieb's study of the exodus of black southerners to Pittsburgh during World War I and the 1920s creatively integrates well-known information about the migratory process into our understanding of black workers and their adjustment to industrial labor. Numerous scholars have discussed the phenomenon of chain migration and the persistence of agrarian habits of work and culture among migrants new to both city and factory. Gottlieb imaginatively and correctly applies these themes to southern black migration to the North, and particularly to Pittsburgh, between 1916 and 1930.

The author places his emphasis on two crucial areas. First, Gottlieb discusses the mechanics of migration. He argues that for most black southerners migration to Pittsburgh and other parts of the North was not a major departure from their recent past. For several years prior to 1916, black agricultural laborers increasingly supplemented their income with work in both light and heavy industry in the nearest southern town or city. As the network of persons with information about and exposure to jobs in nonagricultural pursuits broadened to include additional entrepots in both the South and the North, especially during World War I, migration to such northern cities as Pittsburgh became less intimidating to prospective black migrants.

Second, Gottlieb argues that southern black culture and work habits strongly influenced and decisively shaped the early experiences of black migrants in Pittsburgh. The act of migration itself, whether from a small farm to a nearby town or from the Deep South to the urban North, was a declaration of independence. Workers wished to pursue their own familial and fiscal goals and certainly wanted to determine for whom they would work and under what conditions. Hence, an unusual degree of transiency characterized migrant behavior in the workplace. If a family member was sick at home in the South, if a foreman proved too demanding, or if working conditions turned out to be hazardous, black workers, for these and a multitude of other reasons, would quit their jobs—sometimes after only a few days or weeks as employees. Of course, they knew that other labor-hungry mills in either Pittsburgh or another city would quickly hire them. Self-reliant, independent, and remarkably goal-oriented, black migrants truly wished to "make their own way."

Gottlieb assigns a small role to racial discrimination either as a major factor inducing blacks to migrate or as a significant cause for their persistent presence in Pittsburgh's lower echelon industrial jobs. He carefully chronicles the ugly racist practices of labor unions but neglects to discuss extensively how factory foremen and superintendents drew an occupational line beyond which black workers could not advance. It is almost as though black migrants lived in a world where race mattered very little. Although an unusual degree of mobility during their early years in Pittsburgh conferred some autonomy on black workers, their ultimate economic fate still lay in the hands of white employers.

DENNIS C. DICKERSON
Williams College

JACK TEMPLE KIRBY. *Rural Worlds Lost: The American South, 1920–1960.* Baton Rouge: Louisiana State University Press. 1987. Pp. xix, 390. $40.00.

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Until very recently, historians have generally ignored the twentieth-century rural South, focusing instead on the significant and complex problems associated with the emergence and disintegration of slavery. Except for a few works on the Great Depression and the New Deal years, studies of the twentieth-century agricultural South have been left in part to sociologists. This scholarly vacuum is gradually being filled with the recent appearance of works such as Pete Daniel's *Breaking the Land: The Transformation of Cotton, Tobacco, and Rice Cultures since 1880* (1985), Gilbert C. Fite's *Cotton Fields No More: Southern Agriculture, 1865–1980* (1984), Gavin Wright's *Old South, New South: Revolutions in the Southern Economy since the Civil War* (1987), and, most recently, Jack Temple Kirby's *Rural Worlds Lost*. Taken together, these are welcome additions to the study of southern rural life and culture.

The present book focuses on the process of "modernization" that Kirby sees as transforming the South from 1920 to 1960. Taking issue with the "New South" prophets of Henry Grady's generation in the late nineteenth century, Kirby argues that industrialization and urbanization did not occur in the South until after World War II. Indeed, he says that the "rural South in 1920 was much as it had been in the 1890s; in fact, it was in certain respects worse off," as plantations were larger, the soil eroded, and the cotton economy in shambles (p. xiv). It took the Great Depression, the New Deal, and World War II to destroy the old plantation system, ushering in a diversified, capital-intensive agriculture and a more industrialized and urbanized society. For Kirby, these vast changes represented a watershed in southern history, and his book is an effort to describe these structural transformations and the ways in which they transformed southern culture and communities.

In describing the rural South, Kirby is careful to sort out the different subregional economies and cultures that ranged from the rapidly expanding large-scale plantations in the Mississippi Delta to the Piedmont Region, where largely white tenants eked out an existence in a flagging cotton and tobacco economy. But the most "primitive" parts of the South were the "self-sufficient" Appalachian and Ozark mountain regions. This agricultural South "cannot be described as modern in any way, either in the very old sense of being part of the plantation society and the original world economic order or in the new sense of being well capitalized, at least partly mechanized, and connected to metropolitan markets" (p. 45).

This regional diversity aside, the changes wrought by the New Deal and World War II ultimately led to a "modern" South that was no longer the nation's "number one economic problem." Kirby describes the familiar struggles over federal relief and subsidies during the 1930s, the wartime migration to the cities and factories, and the final shift to mechanized, capital-intensive agriculture. These changes were products of the "federal road" to modernization, according to Kirby, and they did not occur without human consequences. A major result of these transformations was the migration of eight million southern farm people by 1960. For Kirby, the southern exodus, in terms of its "revolutionary" effects on agriculture and humanity, was "at least the equivalent of the principal phases of the English Enclosure Movement of the last four decades of the eighteenth century" (p. 276).

One of the contributions of this study is its focus on the lives of rural southerners, their communities, culture, and struggles. Kirby has collected an impressive array of largely unused archival sources that range from plantation and merchant ledgers to family papers and local oral history projects. These sources, combined with the Federal Writers Project oral histories, have aided Kirby in creating a rich portrayal of southern rural life that reflects both its humor and suffering, its strength and humanity, as well as its contradictions and conflicts. Although some historians may disagree with his use and definition of "modernization" and "self-sufficiency," all will acknowledge his contributions to our understanding of twentieth-century southern history.

NAN ELIZABETH WOODRUFF
College of Charleston

EARL BLACK and MERLE BLACK. *Politics and Society in the South*. Cambridge: Harvard University Press. 1987. Pp. ix, 363. $25.00.

Relying extensively on numerical data, including voting returns and attitude surveys, political scientists Earl Black and Merle Black assess socioeconomic change and its political implications in the American South since 1940. They argue that significant partisan realignment or "dealignment" (no party affiliation) resulted as the urban white middle class expanded dramatically and barriers to blacks in public accommodations and voting fell through a combination of pressure from below by Afro-Americans themselves and federal intervention from above. Largely, these changes strengthened the Republican party and, thereby, the two-party system in the South. From a legally segregated, agrarian, monoparty culture dedicated to maintaining the racial status quo, the old South of the pre–World War II era grew by the 1980s to look much like the non-South—in terms of industrialization and urbanization, a competitive two-party system, and the treatment of black people (equality in voting and access to public facilities but persistent inequality in jobs and housing).

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Although southern blacks overwhelmingly vote the Democratic ticket, their mobilization did not strengthen the Democratic party because large numbers of whites left it, and whites make up the vast majority of the southern populace as well as of its voters. As the authors succinctly remind the reader, "the population-based limitations on black political leverage are manifest" (pp. 294–95). Whites left the "Democracy" in part because they were unhappy with changes such as the Civil Rights Act of 1964 and the Voting Rights Act of 1965, but they also departed because Republican economic policies were more consistent with their own "entrepreneurial version of the individualistic political culture."

As supporters of this version of politics, white southerners differ sharply from blacks over the role the federal government should play in the provision of jobs and a good standard of living. Regarding one's stance on this question as the litmus test for determining liberals and conservatives, the authors emphasize the gulf between black and white voters. They mention many areas of agreement but never seriously consider the political possibilities that might flow from them. Similar positions include "warm feelings" toward the police, southerners, whites, blacks, and the military and "cold feelings" toward marijuana users, gays, and lesbians. A majority of both groups also express satisfaction with their states and local communities, and working-class whites and blacks, but not middle-class whites, feel "warm" toward evangelicals.

Another fact that is noted but not discussed in terms of its political implications is the preponderance of white female workers among the new southern middle class. As the authors themselves point out, the expansion taking place in the middle class since the war resulted more from the entrance of white women than white men into it. Are these women married to working-class men? Can one really term a clerical worker (the occupation of most of these women) middle class? Are there religious or cultural symbols that have greater political salience for middle-class females than middle-class males?

Some readers will no doubt complain about the prevalence of numbers and typologies in this work and the absence of specific individuals and concrete issues. Others will deride the almost mechanistic assumption that class determines voting preferences and note the omission of certain factors such as religion. But the authors have done a fine job of exploring the complexities that surround southern change. In their clear and careful assessment, Black and Black have produced a work that has great import for shapers of southern political parties and aspirants for office as well as for scholars.

GAIL W. O'BRIEN
North Carolina State University

ALAN M. WALD. *The New York Intellectuals: The Rise and Decline of the Anti-Stalinist Left from the 1930s to the 1980s*. Chapel Hill: University of North Carolina Press. 1987. Pp. xvi, 440. Cloth $32.50, paper $12.95.

Alan M. Wald, by his own assertion, sets out to rescue a history that has been "vastly misunderstood" (p. 8) by all previous scholars and to redeem contemporary radicals from their errors (pp. 9, 23) by reviving a proper anti-Stalinist Marxism. The resulting book is shaped in its structure, language, and judgments by this conception of historical and political mission.

Wald contends that the New York Intellectuals should be understood as "an outgrowth of the tradition of the anti-Stalinist left" (p. 7). This "tradition" for Wald is virtually identical with organizations and individuals substantially influenced by Leon Trotsky. Thus, the two-thirds of the book devoted to the period from the 1930s to the mid-1940s concentrates on the evidences of young intellectuals temporarily moving close to Trotskyism. The political development of a group that first came together around the *Menorah Journal* in the late 1920s, for example, claims most of two chapters. At times the history of Trotskyist parties takes center stage, most notably in a full chapter ("Cannonites and Shachtmanites") on the party split of 1940. The dilemmas posed by "The Second Imperialist War" (chap. 7) soon led most intellectuals to drift or bolt away from revolutionary ideas, and Wald conceives the postwar period as a rightward march into ideological justifications for American imperialism and class privilege. The largest sections on the last thirty years are given to a respectful but ultimately dismissive account of Irving Howe's career and to an unfettered attack on prominent neoconservatives.

Wald's perspective brings out strongly one part of the political context within which the ideas of the New York Intellectuals evolved. But can preoccupation with a Trotskyist phase that was often quite short supply an adequate definition for the group or a balanced picture of the New York Intellectuals' concerns? With a concentration on political activism, the member of the *Menorah Journal* group who gets the brightest spotlight in the 1930s is not Lionel Trilling but Herbert Solow, who would "retain only the vaguest connections to New York intellectual life" (p. 151). Trotskyist leader James P. Cannon receives lavish attention; Dwight Macdonald's *Politics* gets a paragraph.

With some of his subjects, Wald seems to lack any fundamental sympathy. He acknowledges that many were literary intellectuals yet insists that their literary interests were "secondary" (p. 6). He recognizes that the editors of *Partisan Review* sought to free radical literature from domination by a political

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party but rejects in his analysis both the political independence of the magazine and the literary values that it fostered. Wald is inclined to quote Trotskyist critiques of the *Partisan Review* circle without an equivalent attention to the statements and responses of those being attacked. The result at times is to obscure rather than illuminate the ideas of the New York Intellectuals and the reasons why they moved away from radical politics.

Wald has done detailed research on the anti-Stalinist Left. Nevertheless, his effort to define and interpret the New York Intellectuals almost exclusively in political terms ignores too much both of their experience and of their productive work.

TERRY A. COONEY
University of Puget Sound

KENNETH S. DAVIS. *FDR: The New Deal Years, 1933–1937; A History*. New York: Random House. 1986. Pp. x, 756. $29.95.

In this third volume of his history of Franklin Delano Roosevelt and his times, Kenneth S. Davis moves from the eve of the inaugural of 1933 through the beginning of 1937—a time, he says, when Roosevelt achieved and maintained "a decisive centrality in the historical process of America" (p. 675). The result is mostly another detailed history of the first term of Roosevelt's administration, broken conventionally into four sections that move successively through the Hundred Days, the period of late 1933 and early 1934, the coming of the "second New Deal," and the political triumphs of 1936. Also brought in, however, are glimpses of the state of the world at the time, digressions into what was happening in the intellectual, scientific, and technological spheres, and interludes focusing on Roosevelt's personal and family life.

Davis's account is written primarily from published sources, adds little to the knowledge that most scholars have of the subject, and offers no fresh and exciting revisionist perspective. It is unlikely therefore to be regarded as a major contribution to historical scholarship. It is also not without faults as a useful and timely synthesis. Although it shows familiarity with much that has been written since Arthur M. Schlesinger, jr.'s, *Age of Roosevelt* (1957–60) and makes appropriate changes in Schlesinger's story and themes, it ignores without explanation or comment much recent revisionism on such subjects as the Civil Works Administration, the coming of the Wagner Act, Hooverian activism, and securities regulation. Its great strength is Davis's ability to tell a marvelously engaging and compelling story while keeping his facts straight, and it should have strong appeal for those who would relive the drama of the period and like their history to come alive.

Davis also feels obliged to say something about influences or forces that shaped the period, and, in an early chapter, he identifies three of these: an intellectual force operating through a particular sort of political intelligence, the force of scientific progress and technological development, and the "vibrant personality" of the leader who dominated political life and gave the times their mood and spirit. These, he says, operated on and interacted with each other. But, subsequently, the only one we learn much about is the personality. Roosevelt, as Davis depicts him, had some unlovely traits (among them streaks of personal vindictiveness and unconscious sadism), but he also had a "manifest genius—a unique blend of intuition, calculation, confidence, charm, personal force—for locating, seizing, then actively holding the focal points of countervailing political power" (p. 675). And this he used to turn despair into hope, to show that democracy could cope with crisis, and to pursue policies that can be praised for keeping the "worst" from happening and criticized for their incoherence, their missing of opportunities, and their prevention of basic structural changes.

Readers who appreciate good storytelling, panoramic sweep, and skillful characterization of personality will enjoy this volume and may look forward, Davis intimates, to a sequel in which Roosevelt moves to the center of the world as well as of the national stage.

ELLIS W. HAWLEY
University of Iowa

PATRICK J. HEARDEN. *Roosevelt Confronts Hitler: America's Entry into World War II*. DeKalb: Northern Illinois University Press. 1987. Pp. xii, 328. Cloth $27.00, paper $9.00.

"The purpose of this study," Patrick J. Hearden tells us, "is to explain how and why the United States became involved in the Second World War" (p. ix). The titles of the eight chapters that follow—"The Crisis of Capitalism," "Hull's Vision of Utopia," "Dr. Schacht's Frankenstein," "The Quest for Economic Appeasement," "The Carrot and the Club," "The Nightmare of a Closed World," "Roosevelt's Dilemma," and "The Dream of a Pax Americana"—outline the explanation of the "why."

Hearden attempts to show throughout that economic considerations were paramount in Franklin D. Roosevelt's diplomacy and that "American leaders were primarily concerned about the menace that a triumphant Germany would present to the free enterprise system in the United States . . . Convinced that capitalism could not function within the

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framework of only one country, they chose to fight to keep foreign markets open for surplus American commodities and thereby to preserve entrepreneurial freedom in the United States" (p. x).

The economic factor in American relations with Hitler's Germany (and, of necessity, with Japan as well) has been studied before. Hans-Jürgen Schröder's *Deutschland und die Vereinigten Staaten 1933–1939* (1970) in effect depicted the United States and Germany as predominantly economic rivals and World War II as essentially a trade war. Detlef Junker's *Der unteilbare Weltmarkt* (1975), by contrast, found economic concerns in American foreign policy to be inseparably linked to concepts of security and freedom within the framework of America's national interest.

Although Hearden cites neither of these earlier studies (and there is no bibliography), his conclusions approximate Schröder's. His case for the primacy of economic considerations rests heavily on assertions by such administration figures as Pierrepont Moffat, Adolf Berle, Norman Davis, and Cordell Hull. He draws extensively (at times with illuminating results) on the papers of George Peek and to some extent on the comments of private citizens such as Thomas W. Lamont, John Foster Dulles, and James P. Warburg, whom he categorizes as "business internationalists." Roosevelt, like Hitler, figures prominently in the narrative but provides little direct substantiation for Hearden's thesis. Neither do the standard works by W. L. Langer and S. E. Gleason, Arnold Offner, Robert Dallek, Robert A. Divine, Herbert Feis, and Joseph P. Lash, on which he relies heavily for the explanation of the "how."

On one level the conclusions of this study are a truism: the United States was propelled into World War II because Nazi Germany posed a threat to its well-being, which ultimately depended on the survival of capitalism. On another, however, they are misleadingly narrow. Hearden correctly asserts that "the American decision to enter the war was . . . based much more on economic considerations and ideological commitments than on either moral aspirations or military apprehensions" (p. x). He later mentions Roosevelt's attempt to sponsor a peace settlement "favorable to American economic interests and ideological persuasions" (p. 223).

But in the study itself the emphasis is so doggedly on narrow economic considerations that the ideological aspects never emerge. When Woodrow Wilson spoke in 1917 of making the world safe for democracy, he meant for representative government, individual freedom, and capitalism. Twenty years later his successors perceived the threat from Germany in much the same way and acted accordingly. That during the depression they talked more often in economic terms is hardly surprising. The essential package remained the same, and Hearden has explored it only in part.

MANFRED JONAS
Union College

J. GARRY CLIFFORD and SAMUEL R. SPENCER, JR. *The First Peacetime Draft.* (Modern War Studies.) Lawrence: University Press of Kansas. 1986. Pp. xv, 320. $29.95.

This is a well-researched, finely nuanced account of the shaping of the Selective Training and Service Act of 1940. The genesis of this significant prewar legislation lay not in the White House or War Department but rather in the hands of an elite private group, the National Emergency Committee of the Military Training Camps Association (MTCA). J. Garry Clifford and Samuel R. Spencer, Jr., delineate how this group, faced with wariness by Franklin D. Roosevelt's administration, took the lead in drafting conscription legislation, arranging its introduction in Congress, and influencing the public debate. What began as a lobbying effort by outsiders took on a different character when Henry Stimson and Frank Knox, MTCA members, assumed key positions in President Roosevelt's cabinet.

Clifford and Spencer clearly depict the various forces engaged in the summer-long battle to enact peacetime conscription. Isolationists and pacifists forged alliances with those who wished to give volunteering a final test before adopting the draft. Proponents of selective service had their arguments buttressed by the dire news emanating from Europe. President Roosevelt slowly came to a more assertive endorsement of the draft, aided in that election year by the Republican nominee's similar judgment. Clifford and Spencer persuasively describe the pressures that led to the abandonment of an American tradition and the enactment of the first peacetime draft.

The monograph is a seamless rendering of two distinctive research efforts. Spencer began in 1947 to collaborate on the memoirs of Grenville Clark, the principal individual in the lobbying effort for the draft act of 1940. The memoirs never were completed, but Spencer drew on the Clark papers to write a first-rate dissertation on the 1940 act in 1951. Clifford, whose study *The Citizen Soldiers* (1972) traced the early history of the MTCA, has taken Spencer's dissertation, exhaustively researched all the relevant manuscript collections, and written an excellent interpretive analysis of the emergence of peacetime conscription.

JOHN O'SULLIVAN
Florida Atlantic University

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JONATHAN E. HELMREICH. *Gathering Rare Ores: The Diplomacy of Uranium Acquisition, 1943–1954*. Princeton: Princeton University Press. 1986. Pp. xiv, 303.

National competition for vital natural resources—such as petroleum, chromium, and iron ore—has fueled international political rivalries and shaped the outcome of conflicts through much of the industrial age. Now, in a fascinating volume, Jonathan E. Helmreich demonstrates how competition for atomic materials influenced cold-war foreign relations.

It is a familiar story how separate research on atomic fission in Britain and America led to Anglo-American cooperation, consummated at the Quebec Conference in August 1943. Less familiar than the frantic efforts to develop the atomic bomb was the frenzied search for uranium and thorium, the two presumably scarce materials considered vital to atomic weapons. The Combined Policy Committee, which reported to War Department Secretary Henry L. Stimson, established a private corporation, the Trust, to purchase and control thorium and uranium outside the United States, the United Kingdom, and the British Commonwealth.

Through this vehicle General Leslie Groves, the American representative and head of the Manhattan Project, set out to acquire with his British and Canadian partners a monopoly of world atomic materials. From Belgian exile sources they obtained enough Congo ore to sustain wartime bomb research. Later, in September 1944, they negotiated a secret ten-year supply agreement with Belgium that provided the critical uranium vital to the U.S. nuclear deterrent in the late 1940s and 1950s. Other preemptive agreements were reached with Brazil and the Netherlands, but not Sweden.

As it turned out, atomic materials were in abundant, not scarce, supply, and Groves's efforts to establish a monopoly were unsuccessful. Nevertheless, Helmreich holds that the efforts at preemption may have succeeded "for at least a few years" in starving Soviet nuclear weapons production, although the program was not intended to prevent the Soviets from gaining the bomb.

This book represents an important contribution to the history of the period. Nonetheless, I have two concerns about the author's perspective. First, given the context of World War II and the struggle against Nazism, the author seems overly critical of War Department officials negotiating secret accords without the knowledge of either the State Department hierarchy or elected officials in Congress. He does acknowledge, however, that one key elected official, President Franklin D. Roosevelt, was informed and that individual diplomats were involved from time to time, including Secretary of State Edward R. Stettinius in Brazilian negotiations.

Second, although the book is soundly rooted in relevant diplomatic and military primary sources, Helmreich apparently has overlooked the rather extensive literature on resource diplomacy. These include the important writings of economic geologist Charles K. Leith and works on the successful efforts of both Britain and the United States to deny other strategic resources to Nazi Germany and the Soviet Union during the same time period. Greater familiarity with these materials would have enriched his discussion of the context for the diplomacy of uranium acquisition.

ALFRED E. ECKES
U.S. International Trade Commission
Washington, D.C.

FRASER H. HARBUTT. *The Iron Curtain: Churchill, America, and the Origins of the Cold War*. New York: Oxford University Press. 1986. Pp. xiv, 370. $24.95.

The title is taken from Winston Churchill's speech in Fulton, Missouri, on March 5, 1946. Fraser H. Harbutt argues that the speech represented a turning point in postwar U.S.-Soviet relations. Harry S. Truman's administration, increasingly uneasy about Soviet behavior, still pursued a policy of accommodation because public opinion, though increasingly anti-Soviet, also still sought peaceful relations with Moscow and continued to believe in the United Nations. Churchill's speech was thus a trial balloon to see if public opinion would accept a more confrontational approach. Indeed, two days after Churchill met with Truman informally on February 10, American policy began to stiffen, and, despite later strong criticisms of Churchill's address, it first publicly displayed its new posture in the UN—now transformed into a U.S. instrument of containment—over Soviet efforts to turn Iran into a satellite. Thus, the cold war began in February 1946, not March 1947 with the Truman doctrine; and it began not over Eastern Europe, an area largely ceded to Stalin during the war, but over Stalin's efforts to expand beyond Eastern Europe.

Not only did Churchill's influence and speech transform U.S. policy but the results represented the fulfillment of two long-held Churchillian aims—opposition to Communism and an Anglo-American alliance—which had eluded him during the war. Harbutt emphasizes that the Grand Alliance during that conflict was anything but unified. The Anglo-Soviet relationship focused on such concrete issues as territorial disputes, client states, and spheres of influence that would determine Europe's future and the historical rivalry along the northern tier from Greece to Iran. The Anglo-American alliance largely centered on joint military strategy. The U.S.-Soviet association, however, was mainly "intermit-

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tent" and revolved largely around the statement of broad principles and the establishment of the UN. Otherwise, the United States was concerned not to provoke Stalin by confronting him with a joint British-U.S. front and not to become entangled in postwar European affairs on the assumption that public opinion would not support such involvement. Thus, by isolating Churchill, the United States ensured that Stalin, conscious of Britain's declining power, would exert pressure on London along the northern tier, thereby precipitating the confrontation it had wished to avoid.

I question the 1946 date for the cold war's beginning. Although U.S. policy did stiffen, the massive demobilization continued and the Republican Congress elected in November took, in Republican Senator Henry Cabot Lodge's words, the meat axe to the budget, including the military services. It is one thing to toughen language and even policy on specific issues, another to sustain that policy over a longer period with resources and a willingness to take risks. That occurred in 1947 when Truman was told that to mobilize public opinion he had better "scare the hell out of the people." Public opinion had remained divided until it was summoned to a new crusade after Britain's collapse.

Futhermore, even if Churchill had never given his speech, the cold war would have erupted. If Iran had not been the immediate cause, "another street car," as McGeorge Bundy once phrased it, would have come along. For the basic cause of the superpower rivalry was that of preserving the European balance of power, which had twice earlier pitted the insular power (Britain) against the "heartland" power (Germany); in both cases, as Harbutt himself states, Britain's declining power had required American help to contain the expanding land power. The postwar international system repeated this pattern as it became increasingly bipolar. Washington was ready for Churchill's warning and call to arms, as it was to George Kennan's famous "long telegram" in February.

Finally, it is unclear whether Harbutt is merely a dispassionate scholar or critical of the United States for finally succumbing to the Churchillian definition of the Soviet threat. Although blaming Stalin for the cold war, he nowhere speculates whether a wartime common front would have initiated that rivalry even earlier or would have made Stalin more accommodating. After all, Stalin knew how weak Russia really was as the war was ending and how immensely powerful the United States was. Only America's leaders appeared unaware of their leverage and the potential benefits of a close U.S.-British partnership—until 1946–47. Yet Harbutt never criticizes what he repeatedly refers to as U.S. "detachment" from Britain.

Despite these criticisms, this is clearly a well-researched book, and the author has obviously thoroughly re-examined the sequence of events and placed the beginning of the cold war in a broader than the usual "Americocentric" framework. The book is well worth reading.

JOHN SPANIER
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DAVID W. SOUTHERN. *Gunnar Myrdal and Black-White Relations: The Use and Abuse of* An American Dilemma, *1944–1969*. Baton Rouge: Louisiana State University Press. 1987. Pp. xviii, 341. $35.00.

In this volume David W. Southern expertly describes and analyzes the protracted and troubled making of Gunnar Myrdal's *An American Dilemma* and the impact of this classic on black-white relations in the United States. Based on meticulous research in an impressive variety of manuscript and printed sources and written with grace and clarity, Southern's work traces the direct and indirect routes by which "Myrdal's ideas got into people's heads" (p. xv) and explains how these ideas influenced, both positively and negatively, racial perceptions and events for more than two crucial decades after World War II.

In 1938 Myrdal, a Swedish economist of great intellectual breadth, accepted an invitation from the Carnegie Corporation to undertake a study of the "Negro problem" in the United States. Lavishly financed by the corporation, his study was actually a collaborative enterprise involving numerous social scientists. But the massive, data-filled work bore throughout the unmistakable stamp of Myrdal, an exuberant optimist for whom social engineering was a higher priority than the quest for objective social science. He early concluded that the race problem was essentially a white problem that resulted from a conflict between verbally honored egalitarian ideas and the racist treatment of blacks. Although posing a wrenching "dilemma" for white Americans, the conflict could be resolved through gentle social engineering without profound structural changes in society. That Myrdal wrapped "his version of 'science' in the American flag and Judaeo-Christian values" made his report all the more acceptable (p. 293). For various reasons, the publication of the study in 1944 could scarcely have occurred at a more propitious moment to achieve maximum effectiveness.

According to Southern, Myrdal's ideas had their most direct influence in the decade following publication. They provided important individuals, private organizations, and government agencies with a compelling conceptual framework and a wealth of data on the race problem. College students of that

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era, including many who became racial reformers in the Kennedy and Johnson administrations, grew up on an academic diet of Myrdal. By 1954 his name "had become a household word in the halls of justice" (p. 128) as evidenced by the important role that his ideas played in the *Brown v. Board of Education* decision.

In the wake of the decision segregationists and John Birchers singled out Myrdal as a favorite whipping boy, while scholars piled up evidence that rendered many of his ideas suspect. Scholars questioned his rank order of discrimination, disputed his assertion that interracial contact reduced prejudice, and punctured his claim that the educated were not as racist as the less-educated. Despite such assaults, Myrdal's authority remained formidable until the mid-1960s when events and trends caused social scientists to discard his "upbeat model of race relations for the more problematic conflict model" (p. 262). Increasingly labeled a false prophet and purveyor of half-truths, Myrdal came to be held responsible for misleading a hopeful generation by focusing too much on individual prejudice and too little on institutional racism and structural arrangements that kept blacks subordinate.

Southern has exhibited great skill and sensitivity in treating a dramatic instance in which ideas and values did shape external reality. His study not only reveals much about civil rights, race relations, and racial thought in the post–World War II era but also provides rich insight into the personalities and politics of the academic world in the late 1930s and early 1940s.

WILLARD B. GATEWOOD, JR.
University of Arkansas

DANIEL J. NELSON. *A History of U.S. Military Forces in Germany.* (Westview Special Studies in Military Affairs.) Boulder, Colo.: Westview. 1987. Pp. xv, 250. $29.95.

This book requires, so to speak, careful reading of the instructions on the label before using. Those instructions, in the form of a nine-page introduction, explain that the subject is not the U.S. military forces as such but rather the American "military presence" in Germany and its significance as a "dimension" in German-American relations. And they disclose as well that Daniel J. Nelson regards the military presence as essentially a backdrop for his "central theme," which is "the progressive development of a relationship of security dependency between West Germany and the United States, with Germany as the dependent partner and the United States as the dominant partner" (p. 3).

Some guidance on organization and methodology is also needed. The first three chapters (of five) cover the 1945–67 period in 85 pages, and the narrative at times becomes kaleidoscopic. Massive retaliation, the New Look, and flexible response, for instance, flash past on 2 pages. The stillborn Multilateral Force plan, on the other hand, gets 6 pages. The final two chapters give the years 1967–85 more extensive treatment (in 104 pages). In them the author shifts from an expository approach based on secondary writings of American provenance to an analytical one predominantly derived from the German press.

Chapter 4, "Deterioration, 1967–1973," is the pivot. In it, owing to the Vietnam War, the partners experience a fall from grace. The Americans' inability to sustain their dominant role in the manner to which the Germans have become accustomed shatters a once mutually satisfactory relationship. Congressional and executive branch attempts to enlarge the German share of European defense costs, doubts about the American commitment to European defense, and American bases that become mere way stations on the route to Vietnam and are populated by loutish, hash-smoking, malcontent soldiers bring about "the nadir of the American military presence" (p. 131).

The post-1973 period, which has not yet ended, is seen as "one of slow and painful reconstruction" (p. 131). The confidence that existed before 1967, particularly on the German side, can probably not be fully regenerated. The security dependency will continue but with a substantially less acquiescent dependent partner. Restoring whatever stability may be attainable will be primarily an American responsibility and will take time, patience, close consultation on vital issues, and no more "carping" about money.

Unfortunately, the most important component in both the American military presence and the security dependency, namely, the so-called nuclear umbrella, does not appear anywhere in the book. That omission leaves three major questions unasked. To what extent did German confidence in the partnership depend before 1967 on an assumption that American strategic nuclear power would promptly and decisively check a Soviet attack? How did the adoption of flexible response as NATO doctrine figure in the subsequent German-American relationship? Can stability be regained under a shrunken nuclear umbrella? Without reference to these questions, in my opinion, a viable history of U.S. military forces in Germany cannot be written.

EARL F. ZIEMKE
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WILLIAM CONRAD GIBBONS. *The U.S. Government and the Vietnam War: Executive and Legislative Roles and Relationships.* Volume 1, *1945–1960*; volume 2,

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1961–1964. Princeton: Princeton University Press. 1986. Pp. 363; 422. $14.95 the set.

Scholars already familiar with the Congressional Research Service project for a thoroughly documented and essentially impartial study of U.S. policy making throughout the period of the Indochina Wars (1945–75) will be delighted to see the first two volumes in the series (down to the end of 1964) being republished by Princeton University Press and so being made available to a wider readership than the original version printed for Congress in 1984. This is not, of course, a comprehensive study of the whole war. We must still rely on other works for any analysis of political and military events in Indochina, and no attempt is made here to cover even U.S. intelligence information about what was happening on the Communist side. The value of William Conrad Gibbons's study is that it follows the course of debate and decisions in Washington with measured thoroughness, presenting much the most satisfactory account so far of that dimension of the conflict.

As the subtitle indicates, the volumes are concerned with executive decision making, the role of Congress, and the relationship between them. In the executive sphere they tread a path already marked out by the compilers of the State Department's *Foreign Relations of the United States* (which are used extensively for the 1950s) and of the *Pentagon Papers* (still very important for the 1960s). But at many points we now have a fuller picture thanks to the progressive declassification of increasingly sensitive documents held by the Eisenhower, Kennedy, and Johnson presidential libraries. The U.S. decision to become directly involved in Indochina in 1950 is notably well covered, as is the relationship between policies in Korea and Vietnam in that year. Also thoroughly treated are Kennedy's decisions of 1961–63 and those of Johnson in the first half of 1964, on which special mention should be made of the treatment of the Laos crisis of spring 1964 (to which the *Pentagon Papers* give far too little attention).

The volumes are especially valuable for their examination of the role of Congress, which was not always very great in the making of decisions relating specifically to Vietnam. (An important exception was the individual role of Senator Michael Mansfield throughout the years 1954–64.) There was little challenge to executive policies on Indochina itself until the Colegrove Hearings on the possible waste of aid to Ngo Dinh Diem's regime in 1959. But Gibbons also explores the congressional contribution to the formulation of the more general principles and legislation of the years 1947–50, which constituted the framework within which executive decisions on Indochina became possible: the Greek-Turkish Aid Act, the China Aid Act of 1948, the first Mutual Defense Assistance Act, and the first Foreign Economic Assistance Act. Not surprisingly, the second volume has a lot to say about the passage of the Gulf of Tonkin Resolution of August 1964. There, too, Gibbons helps place that move into better perspective by indicating, at the appropriate chronological point, the four or five comparable resolutions of the preceding decade—starting with the Formosa Resolution of 1955.

All in all, this is a major contribution to our understanding of the U.S. involvement in Vietnam. It will serve as a valuable work of reference, and those who use it will eagerly await the remaining volumes.

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ALAN B. ANDERSON and GEORGE W. PICKERING. *Confronting the Color Line: The Broken Promise of the Civil Rights Movement in Chicago*. Athens: University of Georgia Press. 1986. Pp. xii, 515. Cloth $40.00, paper $17.95.

The civil rights movement was sustained by its moral force. In this book Alan B. Anderson and George W. Pickering try both to recapture that energy and to renew it not only by providing an analysis of the movement in Chicago but also by proposing a fresh moral assault on the color line in America.

The authors provide an internal perspective, for each was directly involved with the Coordinating Council of Community Organizations (CCCO)—the coalition of civil rights, neighborhood, and professional organizations that served as the vehicle for the local civil rights movement—while attending the University of Chicago's Divinity School in the mid-1960s. They use fresh documentary sources emanating from the movement and provide an insightful glimpse into the convergence of the national and local civil rights efforts and the frustrating attempt to transport the movement's early success to the North. In so doing, the authors resuscitate the concept of the "color line" to deemphasize the increasingly questionable distinction between de jure and de facto segregation.

The *Brown v. Board of Education* decision and massive demographic shifts set the stage for the challenge to Chicago's segregated schools. Black dissatisfaction and the unresponsiveness of Superintendent Benjamin C. Willis sparked the coalescence of the CCCO and culminated in two massive, but unavailing, school boycotts. Stalemated, the Chicago movement attracted the attention of Martin Luther King, Jr., and the Southern Christian Leadership Conference (SCLC) as they moved north and tried to address broader "ghetto issues." In dense,

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sometimes repetitious, detail the authors reconstruct the halting emergence of the Chicago Freedom Movement out of the CCCO and SCLC, the ideological sparring within the movement, the summit conference between Mayor Richard J. Daley and King, and the failure to create an "open city."

A recurring theme is the movement's inability to develop a focused program to confront the metropolitan color line. An overwhelming rush of events simply outstripped its institutional capacity and ability to respond creatively. Violence in the streets transformed Chicago into the North's Birmingham but ultimately failed to facilitate any programmatic thrust. In the end Daley blunted civil rights efforts and contributed mightily to the movement's "collapse."

The authors capture the dynamics of the movement better than they do its opposition. Knowledgeable Chicagoans will be able to discern patterns and meaning in Daley's actions that are not made explicit—others will be at a disadvantage. The relative slighting of electoral politics also precludes a real understanding of what Daley was about and is reflected in the egregiously erroneous assertion (pp. 143, 148) that blacks constituted 58 percent of Chicago's electorate in 1964.

Finally, the authors set their discussion in an intellectual framework that insightfully depicts the ascendance of a "civic credo" that prized peace above justice. That approach ignores, however, the continued economic, demographic, and historical analysis necessary to sustain the book's more questionable prescription for a renewed moral challenge to stubbornly persistent racial distinctions. Undoubtedly a major source of new information and thoughtful analysis regarding the Chicago civil rights movement, this book is an essential but incomplete foray into contemporary race relations.

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CANADA

A. A. DEN OTTER. *Civilizing the West: The Galts and the Development of Western Canada.* Paperback edition. Edmonton: University of Alberta Press; distributed by University of Nebraska Press, Lincoln. 1986. Pp. xiv, 395. $16.95.

The union of British North America, Confederation, was perhaps the greatest single business opportunity in Canada's history. The present book is a sympathetic look at the activities of two men, Sir Alexander Galt and his son Elliot, who were true insiders to and major beneficiaries of these developments.

Galt senior was a father of Confederation. By the time the first Canadian transcontinental railroad (the CPR) reached the Alberta territory in the 1880s, he had amassed a fortune from his rather notorious land and railroad speculation dealings in the east. Then, as documented by A. A. den Otter, he turned his attention to the western interior. When CPR officials made the "surprise" announcement of their decision to route their line through southern Alberta rather than the seemingly more fertile northern part, Galt and his backers immediately tapped into the immense coal reserves and timber limits of that region. The very special financial and land privileges granted to them for laying a railroad from their collieries to the CPR line allowed Elliot to expand into irrigation and colonization enterprises by the turn of the century, when the prairie wheat boom was in full swing. The story ends just prior to World War I, when the CPR bought out these profitable businesses.

This paperback edition is an unrevised version of the original 1982 printing and a winner of a Canadian Historical Association certificate of merit for regional history. Den Otter employs somewhat outdated pessimistic assumptions about the historical experience of Plains Indians in the nineteenth century, such as "tragically, a quarter-century of rot-gut had crushed the red man's spirit, and his dignity was further stripped by starvation as the buffalo disappeared" (p. 4). Such notions simply do not stand up in light of the past two decades of scholarship on native American history.

The author interprets Galt's entrance into the western "wilderness" exactly as the Galts and their associates did: as an act of beneficence, a "civilizing mission." The title he chose is not meant as irony. The story he tells is nineteenth-century in vocabulary and spirit. But the true story here is of the spread of industrial capitalism into the plains economy. From den Otter's own research the reader can only conclude that the Galts were tireless captains of industry who sought every possible political and economic advantage of their day.

DIANNE NEWELL
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WILLIAM KAPLAN. *Everything That Floats: Pat Sullivan, Hal Banks, and the Seamen's Unions of Canada.* Buffalo, N.Y.: University of Toronto Press. 1987. Pp. xii, 241. Cloth $30.00, paper $14.95.

The sordid story of how Canada's seamen's unions came to be purged of Communist leadership by an alliance of capital, the state, and Harold (Hal) Chamberlain Banks, an American import given to thuggery and corruption, has long been a well-known and central chapter in the history of the cold

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war within Canadian trade unionism. Less appreciated, however, are the fascinating details of the consolidation of the Seafarers' International Union (SIU) under Banks's leadership, the initial embrace of Banks and subsequent backing away from the ruthlessness of the maritime trades tsar by the Canadian Trades and Labor Congress (TLC), the resulting tussles between the TLC and the American Federation of Labor, the legal proceedings (concentrated in the Norris Commission and the Norris Report) that led to Banks being deposed and the union being placed under trusteeship, and, finally, the political mystique and machination surrounding Banks's all-too-easy flight to the United States and the Canadian state's failure to have him extradited. William Kaplan has filled in many gaps in our knowledge with a book that is at times judiciously researched and at times judicially argued.

The opening sections of the study, on the Canadian shipping industry, World War II and the strike of 1946, and the rise and fall of the Communist party–led Canadian Seamen's Union (CSU) are the weakest in the book. These are areas for which the author's knowledge of the law are the least helpful and his reliance on obvious sources most limiting. In spite of his inclusion in the subtitle, Pat Sullivan remains a shadowy figure in this book, and Kaplan's liberal anti-Communism so pervades discussion of the CSU that he cannot get beyond the stereotype of red trade unionism as sinister conspiracy that was typical of the 1950s. Kaplan is a defender and advocate of "free enterprise" unionism, of labor organization that concerns itself with bread-and-butter issues and avoids all political struggles. I would bet he liked *Solidarność* and Lech Wałęsa, which might give him cause to reconsider his position.

Enter Hal Banks. Because the CSU was Communist-led, and because it was willing to engage in labor-political strike action, it needed to be routed by any means necessary. Banks, according to Kaplan, was no model citizen, but he was the man for this dirty job that someone had to do. And, with the aid of the Canadian state, the shipping companies, and reactionary pillars of the trade union hierarchy, he did, in fact, do it rather well. When his megalomania got the better of him and he continued to act, throughout the later 1950s and into the 1960s, the part of the tyrannical, state-protected, labor-bureaucracy endorsed magnate of the maritime working class, beating his union oppositionists into submission, blacklisting and ruining thousands of seamen, and looting the SIU coffers to support his lavish life style, he too, like the Communists who preceded him, had to be dumped. Ironically, for those SIU members who could work and rocked none of Banks's boats, it was probably a good time to be a seaman, economically, since wages and working conditions in the Banks era were as good as those of the CSU epoch. When, one is tempted to ask Kaplan, does bureaucratic banditry cross the dichotomized economic-political line that he conceives as separating legitimate from illegitimate trade unionism?

Kaplan is addicted to the term "housecleaning." It is curious, however, that it never enters his mind that labor's rank and file should clean its own house. Rather, when housecleaning is called for by Kaplan, it inevitably takes place from the top down, orchestrated by the "good" labor leaders, as opposed to the bad ones such as Banks, and undertaken with the aid of that impartial umpire, the state, and its blindly unprejudiced judicial arm, the rule of law. Unfortunately, as Kaplan's own account reveals, the state is and was during the 1940s, 1950s, and 1960s no impartial umpire, and the rule of law, embedded as it is and was in capitalistic social, economic, and political relations, has proven historically to be little protection for the working class.

A short review such as this cannot pursue the avenues of interpretation not traveled in this study, nor can it address the way other analytical directions are sidetracked. Neither is there space to do justice to the many contributions of detail Kaplan has made, especially in terms of the legal history of Banks's fall from grace. This is a book, for all my complaints, that will have to be reckoned with. It is also a text that raises a rather large question that Kaplan, the legalistic empiricist and ideologically committed champion of business unionism, manages to avoid: when will the considerable price that the workers' movement pays for anti-Communism be recognized?

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LATIN AMERICA

LESLIE BETHELL, editor. *The Cambridge History of Latin America.* Volume 4, *c. 1870 to 1930.* New York: Cambridge University Press. 1986. Pp. xviii, 676. $79.50.

LESLIE BETHELL, editor. *The Cambridge History of Latin America.* Volume 5, *c. 1870 to 1930.* New York: Cambridge University Press. 1986. Pp. xviii, 951. $69.50.

The past two decades have witnessed a quantum leap in our knowledge of Latin America's past and in the sophistication of scholarly approaches to its study. During this time Latin America of the late nineteenth and early twentieth centuries has re-

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ceived considerable attention from both historians and social scientists. These volumes suggest that there have been greater advances in social and economic history than in intellectual and cultural history and that progress in understanding the political history of the era has been uneven.

The two volumes covering 1870 to 1930 offer diverse perspectives on this formative period. The twelve essays in volume 4 treat the era in broad thematic and comparative terms, with chapters discussing Latin America's relationship with the international economy and the great powers, the transformation of rural and urban landscapes, the beginnings of industry and a working class, the evolution of demographic patterns and ideologies, and developments in religion and the arts. The twenty-one chapters of volume 5 survey the histories of the region's twenty sovereign nations plus Puerto Rico, with four devoted to Argentina and three each to Brazil and Mexico.

What emerges from these massive tomes is an era shaped by Latin America's accelerated integration into the global capitalist economy, whose motor was the Industrial Revolution in Europe and North America. It was the "golden age" of export-oriented growth, with material prosperity for local elites, a liberal-positivist ideological consensus, and relative political stability and regional peace. Although few of the authors (twelve each from the United States and Europe and nine from Latin America) embrace dependency theory and several explicitly reject it, the pervasive influence of external factors is clear in their accounts. The international economy penetrated deep into Latin America during this epoch, bringing profound social, political, and cultural changes in its train. Argentina (above all), Brazil, Chile, Cuba, and Mexico led the way, but the other nations of the region were not immune to its effects, as thoughtful essays by Peter Klarén and Juan Oddone on the formation of modern Peru and Uruguay underscore. It is on the impact of this economic integration on the peoples of Latin America that the authors most clearly divide, with some embracing the local elites' liberal vision of general progress, whereas others stress the uneven division of benefits and the social costs of the process for a majority of Latin Americans.

In a brief review it is not possible to comment on all of the thirty-three contributions that constitute these two volumes. The essays in volume 4 are generally of high quality, including those by William Glade and Rosemary Thorp on the international economy, Nicolas Sanchez-Albornoz on demography, and Arnold Bauer on rural society. James Scobie's final statement on urbanization is an excellent one, and special praise is merited for the efforts of Gerald Martin and John Lynch to create syntheses of the region's cultural and religious history despite limited scholarly advances in those fields. Volume 5 begins strongly, with first-rate essays by Friedrich Katz on the *porfiriato*, John Womack on the Mexican revolution, and Jean Meyer on the Sonora dynasty. The country histories that follow are more diverse. Some do little more than revise traditional political chronologies; others, such as Warren Dean's incisive survey of the Brazilian economy and Boris Fausto's illuminating chapter on politics and society in Brazil's Old Republic, are analytic in their approach. Most of the authors are content to summarize current conventional wisdoms, but Emilia Da Costa's suggestive essay on Brazil's "age of reform" (1870–89) breaks new interpretive ground, and Manuel Morena Fraginals's contribution on the plantation economies of the Spanish Caribbean extends his pathbreaking work. Argentina is well represented, although it is difficult to reconcile Roberto Cortes-Conde's optimistic view of the prospects of tenant farmers and urban workers with the more pessimistic visions of David Rock and Hobart Spalding. The essays vary in quality and interest, but the overall impression is one of solid scholarship and sound judgment, a reflection on both the authors and editor Leslie Bethell.

Not all of the editor's choices were so felicitous. Chapters on women, Indians, and blacks are conspicuous by their absence, as are essays on science and technology. Moreover, the whole is somehow less than the sum of its parts. This may be because there is no synthetic overview, because of the authors' divergent orientations, or because of uneven historiographical progress. As a result, the thirty-three essays do not add up to an entirely cohesive view of Latin America between 1870 and 1930.

Few readers, however, are likely to sit down and read these volumes from cover to cover. Rather, their chapters are likely to serve as an indispensable reference for scholars, as a convenient summary of recent scholarship for teachers and students, and as standard statements of current conventional wisdoms against which future research and writing will be framed and judged. In short, these volumes amply justify the vision that launched them.

PETER WINN
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HERBERT S. KLEIN. *African Slavery in Latin America and the Caribbean*. New York: Oxford University Press. 1986. Pp. 311. $22.95.

Herbert S. Klein's broad survey of the history of African slavery throughout Latin America and the Caribbean provides a convenient overview to nonspecialists. It begins with a short explanation of the origins of the American slave system, noting that its most relevant historical antecedent occurred dur-

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ing the Roman empire, when slaves accounted for between 35 and 40 percent of the population, and later with the expansion of sugar production in the Levantine Mediterranean between the eighth and thirteenth centuries. Slavery in African societies before the development of the transatlantic slave trade is also mentioned, though with less specificity than the earlier Roman and Mediterranean varieties. The narration of the changing experience in the Americas is familiar in a wide variety of sources, especially Philip Curtin's *Atlantic Slave Trade* (1969) and Leslie Rout's *The African Experience in Latin America* (1976).

Both the Spanish and the Portuguese employed African slaves in their early colonial ventures in the Americas for a variety of reasons, according to Klein, but mainly having to do with wage rates in Europe. Once in the Americas, African slaves and their descendants performed just about every variety of task required—fighting Indians, building cities, making and selling hats, huckstering, mining for silver. In some places on the mainland Afro-American populations grew at faster rates than the whites or mestizos. But the African demographic expansion responded to the available supply of indigenous Americans, and the adaptability of their social organization to the demands of the new slaveholding society. This can be noted in the varying concentrations of Africanized population elements. Until about the middle of the seventeenth century, Africans comprised between 10 and 15 percent of the Peruvian population, about 50 percent of the inhabitants of the capital, Lima, but only about 2 percent of the Mexican population. An adequate supply of Indians made the introduction of Africans unnecessary. One inevitable consequence of introducing African slaves was the formation of a group of free persons of color that played a significant intermediate role in the social and economic life of the colonies.

If Curtin's *Atlantic Slave Trade* indicated when and how Africans came to the New World, Klein demonstrates what they did, how they lived, and the broader social and economic environment of labor and production that nurtured the transatlantic migration for centuries. This book tries to combine the chronology of development and decline with an examination of germane themes of the American slave system: living conditions, economic forces, mortality rates, family structures and kinship, culture and household arrangements, the role of freedmen, and the transition from slavery to freedom. The attempt is not entirely successful.

In any general study, especially one that ranges as far as this, any number of specific objections may be made to facts and interpretations. Since the book dispenses with footnotes—offering instead a skimpy bibliographical essay for each chapter—specialists will find much to quibble about: the figures of "at least some 20 to 25 million American Indians" as the base population in 1492 (p. 21); the effect of wages on migration and of kin and linguistic factors on African slavery (pp. 23–26); the sugar industry in Puerto Rico (p. 104); the lack of a definition of race (p. 218); and the ambiguity in using the term "freedmen." Nevertheless, this is an interesting addition to the literature.

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JAY KINSBRUNER. *Petty Capitalism in Spanish America: The Pulperos of Puebla, Mexico City, Caracas, and Buenos Aires.* (Dellplain Latin American studies, number 21.) Boulder, Colo.: Westview. 1987. Pp. xxii, 159. $17.95.

Jay Kinsbruner's carefully researched monograph concerning "the very beginning of the entrepreneurial continuum" (p. xvii) describes the incidence, spatial distribution, social origins, organization, capitalization, profits, and problems of small retail grocers in late eighteenth- and early nineteenth-century Spanish America. The analysis is essentially economic, and Kinsbruner carefully disassociates himself from any explicit hypothesis concerning class or development. Instead, he focuses on a kind of "thick description."

The ratio of retail grocers to the population varied in Spanish Amercia from a low in Mexico City of about 1:600 to a high in Caracas of about 1:120. The high ratio in Caracas corresponded to the lowest levels of capital per store and to municipal regulations allowing the sale of wine and liquor. Kinsbruner finds that grocers did not operate as a "class." Often, the larger grocers, in some cases members of the militia or even the city council, attempted to eliminate smaller competitors, usually through municipal regulations forcing a certain level of capitalization. Many times retail grocery stores were family operations with wives and children as key employees. Widows often took over *pulperias* from their husbands, but grocery stores owned by women tended to be capitalized at low levels. Multiple ownership of retail grocery stores was common, and artisans frequently bought retail grocery stores as investments. Profits ranged from 12 to 20 percent of capital, and—although many businesses failed within one or two years—about 30 percent of those examined by Kinsbruner survived for ten years or more.

Possibly, Kinsbruner's most general conclusion is that—because of the absence of banks, joint-stock companies, and other possible avenues of investment outside of land—retail grocery stores were an important focus of investment and entrepreneur-

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ship in Spanish America. By comparison New York retail grocers invested less money in store fixtures, showcases, barrels, and the like and more money in inventory; few New York grocers owned more than one store. Instead, they invested in "income-producing limited liability instruments." Thus, Spanish-Americans were "more entrepreneurial" (p. 104). Kinsbruner states that the book "is a contradiction of the so-called 'dependency' theory" although he does not elaborate this idea (p. 97). Perhaps the point is that most items for exchange were produced locally, but in that case it would also be important to assess the contribution of export production to the incomes of the clients of the *pulperias*.

Kinsbruner's study makes the social and political importance of the *pulperias* obvious. The *pulperias* functioned as social meeting places and as drinking houses—an aspect of the business that bothered municipal councillors. These functions were reinforced by the common practice of using rum as "change" for many transactions, particularly because of the scarcity of coin.

The *pulperias* were usually required by law to serve certain public functions. For example they often had to sell two qualities of bread at prescribed prices, to ensure food to the poor. Furthermore, a law of 1757 required grocers in Mexico City and Puebla to accept "certain items of personal property as pawns in return for comestibles" (p. 56). These items included clothing and jewelry and had to be held by the grocer for one year before they could be sold, thus negatively affecting the grocer's liquidity.

The *pulperias* were not simply "good business"; the municipality regarded them as politically and socially necessary to maintain the exchange economy and to support the poor and the laboring classes. Perhaps *pulperias* were a form of "petty capitalism," but they were also integral to the corporate nature of colonial Spanish society, in which self-contained entities were to interact and cooperate for the general welfare. The "embedded" nature of economy in Spanish American society, combined with business and inheritance laws that hampered capital accumulation until the end of the nineteenth century, makes a purely economic approach to any Spanish American self-employed business group problematical. Nevertheless, Kinsbruner usefully and provocatively portrays a politically, socially, and economically important group in the period of incipient economic and social transformation from a preindustrial to a capitalistic political economy.

ELIZABETH KUZNESOF
University of Kansas

RHYS JENKINS. *Transnational Corporations and the Latin American Automobile Industry*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1987. Pp. xiv, 270. $32.95.

This analysis of the Latin American automobile industry is an important addition to the literature on dependent industrialization in developing countries. Rhys Jenkins's explicitly regional focus and his discussion of Latin America's future role in the world automobile industry are particularly useful contributions.

Jenkins adopts a broad definition of the "motor industry complex" (including automobile manufacturing, parts and tire industries, and distribution networks) as the basis for his study. One of his most significant contributions is a detailed examination of production processes, market conditions and price formation, relations between terminal firms and their suppliers, and profitability and capital accumulation in both advanced industrial and developing countries. Jenkins recognizes labor's central importance in the growth of the automobile industry, and he analyzes labor control strategies, productivity, and wage patterns in distinct national contexts. However, the depth of his understanding of labor-management relations in Latin America varies greatly with the quality of available secondary literature. Jenkins also includes chapters on the international evolution of the automobile industry and its development in Latin America, but much of this material appeared in his earlier books on the subject.

Jenkins emphasizes the interplay between state development policies and transnational corporations' global strategies in shaping the automobile industry in different Latin American countries. His discussion of events in the 1970s focuses on three sets of countries and the distinctive approaches they pursued: export promotion in Brazil and Mexico, market liberalization in Chile and Argentina, and (unsuccessful) import substitution in the Andean Pact. By the 1980s, however, Latin American strategies evidenced the common impact of structural changes in the global automobile industry, including the progressive internationalization of capital, heightened competition, and the homogenization of markets and product demand. Jenkins argues that future developments in Latin America (particularly in Brazil and Mexico) will reflect the region's close integration in the world automobile industry and the predominance of transnational corporation-controlled export production. Latin America's prolonged economic crisis is likely to be a central factor in shaping future trends, but Jenkins offers few comments on this subject.

This is a stimulating and insightful examination of the Latin American automobile industry's historical development and probable future course. The

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book is generally well organized and based on thorough research.

KEVIN J. MIDDLEBROOK
Indiana University

RONALD KENT RICHARDSON. *Moral Imperium: Afro-Caribbeans and the Transformation of British Rule, 1776–1838*. (Contributions in Comparative Colonial Studies, number 22.) Westport, Conn.: Greenwood. 1987. Pp. viii, 211. $35.00.

Why did slavery become an acute problem for British imperial policy in the late eighteenth century, and why was it abolished by the 1830s? The present book seeks to uncover the social forces behind the antislavery ideology. Dissatisfied with previous idealistic and economically oriented explanations, Ronald Kent Richardson offers a "strategic" theory of abolition. The process was a dialectical interchange between two classes of actors, the increasingly restive slaves in the Caribbean and the "Saints" in Britain. As Caribbean resistance threatened to merge with French revolutionary violence, antislavery became part of a Claphamite cultural counterrevolution at home and campaign for reform abroad. The relative threat of resistance, concludes Richardson, explains why the Afro-Caribbeans, and not the Irish or the Indians, became the target of imperial anxiety.

The hypothesis of a strategic counterrevolution evokes a host of unanswered questions. Black slaves in England (not the Caribbean) and those being enslaved in Africa were the principal targets of abolitionist concern during three-quarters of Richardson's chronological period. In neither of these areas was abolitionism a response to revolutionary threats, and the African slave coast was not even part of the British empire. Before 1792 the abolitionist movement paid virtually no attention to the threat of slave resistance, and the uprisings on St. Dominique and elsewhere in the Caribbean had a very ambiguous impact on the timing of the abolition of the slave trade, the exclusive focus of the Claphamites before 1815. Finally, in viewing slave resistance as the primary source of imperial anxiety, Richardson offers no compelling argument as to why the turbulent Irish should not have been perceived by the elite as far more threatening to imperial stability in the generation after 1776.

On the metropolitan side of his bipolar dialectic Richardson explicitly reduces abolitionism to the Clapham sect. His one passing reference to a mass popular movement (p. 99) misdates its emergence by more than a generation. In striking contrast to the Afro-Caribbeans, the British people are presented as a *tabula rasa* on which the Claphamites, a close-knit elite near the "pinnacle of imperial society" (p. 103), inscribed their ideological counterrevolution. The history of slavery and antislavery is therefore a struggle involving white elites and black masses (pp. 15, 41). Other actors are simply excluded. It is also puzzling, in a comparative history series, that the book does not ask why colonial resistance to French, Dutch, and Spanish dominion found fewer abolitionist echoes among Continental than among British elites for the period 1776–1838 as a whole.

Finally, one may reasonably ask why Richardson does not refer, either in his text or in his bibliography, to recent alternative approaches to abolition or to a number of collective volumes and essays on the history of British slavery. Howard Temperley's uncited "Antislavery as a Form of Cultural Imperialism" (in C. Bolt and S. Drescher's *Anti-Slavery, Religion, and Reform* [1980] pp. 335–50) foreshadows Richardson's title with a very different thesis.

Richardson's book is of interest as a novel attempt to place Afro-Caribbean history at the center of the entire abolition process. It emphatically succeeds in showing that there was an element of strategic threat in antislavery ideology. It has yet to be demonstrated that what was chronologically and explicitly an appendage to the antislavery argument was the principal catalyst in the emergence of the abolitionist movement in the 1780s or in the shift to emancipation in the early 1820s. There may be a better case for enhancing the role of slave resistance in the termination of slavery and apprenticeship for the years 1833 to 1838.

SEYMOUR DRESCHER
University of Pittsburgh

LOUIS A. PEREZ, JR. *Cuba under the Platt Amendment, 1902–1934*. (Pitt Latin American Series.) Pittsburgh: University of Pittsburgh Press. 1986. Pp. xvii, 410. $39.95.

This masterly account of Cuba's history during the first third of the twentieth century links the diplomacy of U.S.-Cuban relations to the consequences of those relations for Cuba's internal affairs. Based on extensive archival research in the United States and in Cuba, the book focuses on the Platt Amendment (named after U.S. Senator Orville Platt). The Platt Amendment was appended to Cuba's constitution in 1901 as a precondition for the United States to grant Cuba its independence in 1902, and it was the essence of the U.S.-Cuban Permanent Treaty of 1903. The Platt Amendment gave the U.S. government the right to intervene in Cuba's international and domestic affairs at its discretion; the Permanent Treaty could only be modified by the consent of both governments.

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Louis A. Pérez, Jr., shows effectively that the Platt Amendment promoted the very conditions that its framers sought to discourage: instability in Cuba and threats to U.S. property. When opponents of a Cuban president felt stymied, they appealed to Washington for relief. If Washington paid no attention, the Cuban opposition took up arms and destroyed some U.S. property, whereupon the U.S. government either reoccupied Cuba (replacing its government, as happened from 1906 to 1909), orchestrated the overthrow of the president (as happened in August 1933 and January 1934), or took other measures to interfere in the routine affairs of its Cuban protectorate—such as taking charge of the Cuban population census, opposing changes in the marriage laws, or approving and vetoing appointees to the Cuban cabinet. Conscious of these self-defeating consequences of the Platt Amendment, from 1902 to 1934 the U.S. government tried at least five different interpretations of the amendment, which led to quite different U.S. policies but to comparable consequences in Cuba. None achieved U.S. goals. The two governments repealed the Platt Amendment in May 1934.

Without detracting from the book's merits, I must point out problems in Pérez's treatment of some secondary themes. For example, in part because he relies extensively on State Department correspondence, the role of the president of the United States is not always clear, and that of Woodrow Wilson is invisible. Moreover, Pérez's portrayal of Cuba's political parties makes them appear rather coherent in internal discipline and in social base. In fact party organization was quite fluid; yesterday's enemies often allied the day after; the programmatic content of the parties was fuzzy at best; and the social bases of party power changed accordingly. In addition, Pérez gives too much credit to Cuban President Gerardo Machado for some nationalist positions (opposition to the Platt Amendment and approval of a tariff law to protect infant industries). In fact Machado did little to get rid of the Platt Amendment, and his tariff strategy was more ambiguous.

Graciously written, cogently argued, richly documented, clear, shrewd, and insightful in its analysis of both political and social history and building on Pérez's previous book on Cuban history from the 1880s to 1902 (although the current work stands by itself), this book confirms Pérez as the leading historian of early modern Cuba.

JORGE I. DOMÍNGUEZ
Harvard University

JAMES L. DIETZ. *Economic History of Puerto Rico: Institutional Change and Capitalist Development.* Princeton: Princeton University Press. 1986. Pp. xxiii, 337. Cloth $65.00, paper $20.00.

As everyone knows, modern Puerto Rico is an unincorporated dependent territory of the United States in which, politically, a range of congressional committees and federal regulatory agencies controls many important areas of government without any real Puerto Rican participation and in which, economically, chronic dependency is exacerbated by the enormous transfer of federal funds, notably through the federal food-stamps program, which helps sap the work ethic in the local working population. One can almost say that Puerto Rico is a society in which there is a ruling class that does not rule and a working class that does not work, thus making it a curious anomaly in the long history of colonialism in the Caribbean region.

James L. Dietz has written a remarkably comprehensive account of the historical background to all this, using the latest theoretical concepts in economic science to buttress his case that the industrialization program of the last thirty years has essentially failed to solve the problems of persistent structural unemployment, continuing inequities in income, escalating consumerism followed by massive personal indebtedness, and continuing structural dependency on the U.S. market system. Dietz fairmindedly acknowledges that the social welfare programs have improved social services, health, education, and housing for the ordinary citizen, and there is no longer the massive earlier poverty portrayed in the book's photographs. However, the book's main thrust is to emphasize the high price that the Puerto Rican people have had to pay for their new condition. "Operation Bootstrap (the industrialization program)," he writes, "then is a monument not to economic progress but to the costs and dangers inherent in a development program based upon capital-intensive, foreign-owned, vertically-integrated, and export-oriented corporate expansion. This experiment has been carried out more fully in Puerto Rico than in any other country in the world, and the island's experience should serve as a lesson for other nations in what *not* to do" (p. 309). In this sense Dietz writes not as a conventional economic historian but as an observer with a social conscience. He obviously remembers the observation of the British Fabian political sociologist Beatrice Webb that the technologically possible is not always the ethically desirable. He also has a keen sense for the distribution of economic wealth and power in the "new" Puerto Rico, evident enough in his description of the economic power of the investing transnational corporations in the island economy (pp. 252–55) as compared with the weak and vulnerable position of the local trade union movement (pp. 221–23). The name of the game in this much applauded developmental "model," clearly enough, is powerful business and powerless labor.

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Dietz's book appears at the same time as another study covering much of the same ground, Fernando Pico's *Historia general de Puerto Rico* (1986). My only regret is that both of them competently describe the disease but neither of them in any real way offers a prescription for cure. Dietz simply ends with the not too helpful comment that for Puerto Rico the future is not bright. I would have liked to have seen him, as an economist, offer some alternative developmental policy that might make things brighter. It is true that some alternatives might seem utopian, as, for example, the argument in yet another book—Richard Weisskoff's *Factories and Food Stamps: The Puerto Rican Model of Development* (1985)—that Puerto Rican economic planning must go back to a rejuvenated agricultural plan, so that at least the Puerto Rican people can begin, more sensibly, to feed themselves rather than suffer from a massive food-import bill. Weisskoff may be wrong, if only because industrial machine technology, whether under capitalist or socialist regimes, everywhere destroys agriculture. But at least it is an arguable alternative, worthy of debate. I only wish, after reading this admirable book, that Dietz had attempted to write his own alternative program. I hope he will do that in his next book.

GORDON K. LEWIS
University of Puerto Rico

JOHN TUTINO. *From Insurrection to Revolution in Mexico: Social Bases of Agrarian Violence, 1750–1940*. Princeton: Princeton University Press. 1986. Pp. xvii, 425. $42.50.

John Tutino's study is an ambitious, stimulating, and generally successful attempt to analyze the history of agrarian change and peasant protest from late colonial to revolutionary times. The theme, as vast as it is vital to any understanding of Mexican history, requires some carefully thought out "organizing concepts," and Tutino initially advances four key variables that he sees governing peasant responses to agrarian change (he does not attempt any comparable theorization of agrarian—or economic—change per se; his concluding comments on the penetration of capitalism are brief and unhelpful). Peasant protest—or quiescence—is explained in terms of different combinations of these variables (material conditions, autonomy, security, mobility), as exemplified in concrete regional and historical cases. Unlike some authors, who promptly forget their theoretical prolegomena once they have entered the thickets of empirical history, Tutino remembers, rehearses, and refines his categories, offering an analysis that is lucid, informative, and, at times, ingenious. His schema enables him to avoid simple occupational pigeon holing (conservative kulaks, revolutionary "middle peasants," and so on), and, following those who have rightly spurned crude behaviorism, he shows a due regard for subjective factors (such as peasant sentiments of "moral outrage").

Given the scope of his study, however, he leans to broad generalizations, and these are of a broadly structural-materialist kind. Peasant protests stem from major socioeconomic pressures, related to economic, demographic, and macropolitical changes (such as independence). Little attention is given to specific political conjunctures, such as the Mexican-American War, or to possible psychohistorical explanations of the kind now being advanced apropos the insurgency of 1810. There is, in other words, a tendency toward economic reductionism—logical and defensible in some respects but clearly excessive when, for example, the author qualifies the Cristiada as a "major agrarian [not rural] insurrection," motivated by "bluntly agrarian" demands (pp. 343–46).

Peasant protest thus faithfully mirrors the epochal phases of Mexico's socioeconomic history. The independence movement is seen as the outcome—diverse, complex, but logically explicable—of the preceding years of economic transformation, which bore down hard on the estate workers of the Bajío, the villagers of Jalisco, the remoter peasant communities of the Mezquital and Sierra Gorda, but which, relatively speaking, spared the central highlands or at least gave hardship an appearance of divine rather than human instigation (thus favoring social quiescence). Weaving together a skein of threads—elite divisions, the vicissitudes of mining and manufacturing, the incidence of drought and dearth—Tutino here presents a masterly analysis of agrarian Mexico on the eve of independence. In this context, however, he has the benefit of some excellent prior research (including his own), and he devotes more space to this 66-year period (1750–1816) than to the 124 years that followed (1816–1940). This imbalance is also reflected in the quality of the analysis. In tackling the period 1816–80—one of agrarian "decompression," marked by a flaccid market, an economically declining landlord class, a weak but landlord-controlled state, and a resurgent peasantry—Tutino has to venture a general synthesis on the basis of flimsier evidence. The broad outlines of the analysis are convincing, but particular elements, especially the more innovative, remain conjectural, if only because they derive from single cases. "Landed elites," we are told, "convinced themselves that they were being besieged by insubordinate peasants" (p. 236): a subjective assertion hard to prove and, indeed, far from proven in the text. Yucatecan landlords are said to have deployed state power in lieu of economic power, but, again, the case remains murky. It is also not clear why the 1840s saw such

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novel and widespread peasant revolt. Tutino, following his structural-materialist approach, has to see this as a (strangely?) belated response to the sociopolitical effects of independence (which is no doubt partly true), but some would prefer to stress the more contingent effects of war, political upheaval, and arbitrary taxation and recruitment.

The final section of the book, dealing with the Porfiriato and revolution, is arguably the weakest, especially given the relative abundance of sources for this period. Tutino rightly regards the Porfiriato as a phase of renewed agrarian "compression" (market and estate expansion, peasant dispossession, declining rural living standards), which debouched into the popular upheaval of 1910–20. But his analysis of Porfirian agriculture, heavily dependent on limited (though good) sources, is questionable in several particulars. The importance of grain imports is overemphasized, and that of tariff protection ignored; coercive labor recruitment is probably exaggerated; and exports are equated with profits whereas production for the domestic market is discounted. Finally, in presenting a simple, sweeping, and essentially traditional, agrarian-populist view of the revolution, Tutino assumes a position that, though fundamentally correct, is highly vulnerable to revisionist criticism, which has come thick and fast in recent years, and which he largely fails either to confront or to counter (hence, he unfortunately makes future revisionist point scoring rather easier). For example, he readily qualifies Villismo as a "massive agrarian mobilization"; he attributes Villa's appeal to agrarianism and his defeat to the lack of concrete agrarian measures; he hypothesizes that a Villista victory might have led to "a more radical outcome, a more complete agrarian triumph" (pp. 298, 305, 309, 361). And, as I have noted, he appropriates the Cristeros for the agrarian cause.

These accumulating dubieties, although they mar the last third of the book, by no means invalidate it. The theoretical framework stands up throughout, and, even where he is open to empirical question, the author rarely fails to illuminate and stimulate. Perhaps conventional academic discretion might have dictated that he limit his study to the earlier, better researched period of the late colony and insurgency, which here receives expert analysis. But discretion is not necessarily the better part of historical valor, and Tutino should be applauded for attempting a grander, bolder synthesis that, faults notwithstanding, will become central to future discussions of modern Mexican history.

ALAN KNIGHT
University of Texas

DAVID W. WALKER. *Kinship, Business, and Politics: The Martínez del Río Family in Mexico, 1823–1867*. (Latin American Monographs, number 70.) Austin: University of Texas Press. 1986. Pp. vii, 278. $27.50.

David W. Walker's study is a welcome addition to the economic history of early republican Mexico. This scholarly monograph on the Martínez del Ríos, owners of one of the great family firms of the day, illuminates better than anything so far the day-to-day difficulties of doing business in Mexico during the turbulent half-century after independence. Using a variety of primary source materials—including the family's private correspondence and business records, notarial documents, judicial proceedings, and the archives of the Mexican Ministry of Foreign Relations and the British Foreign Office—Walker meticulously traces the family fortunes since their arrival from Panama in 1823 until the bankruptcy of the family firm in 1864. An epilogue sketches the family's economic recovery and reverses into the twentieth century.

Because the family's holdings were diversified, this study covers international trade, mining, textile manufacturing, money lending, speculation in government bonds, and real estate. Walker expands the picture of these enterprises provided by historians such as Robert Potash, Jan Bazant, Robert Randall, Margarita Urías, and Barbara Tenenbaum. In the process he provides fascinating descriptions explaining, for example, how *libranzas* worked in lieu of cash, how difficult it was to buy land prior to the Reforma, and how the Mexican-American War affected Mexican businessmen. He shows how profits in each economic sector depended on fluctuating government policies and how constant changes in leadership coupled with the government's insolvency undermined the stability necessary to long-term planning until, by the late 1840s and 1850s, there simply was no secure investment in Mexico. In the end land, which the Martínez del Ríos acquired as an afterthought, proved the safest venture, although its rewards were reaped only during the Porfiriato.

Walker's study shows how fruitful it can be to focus on the family as a unit of analysis. Although he is more interested in the family's finances than its private life, he provides intriguing glimpses into the Martínez del Ríos' marriages, education, relations with compadres, and social life. He finds that a major weakness they did not overcome until the third generation was the lack of family connections in Mexico. The founder, arriving a widower with six children, attempted to marry into a well-to-do Mexican family, but his marriage soon ended in an ecclesiastical divorce that prohibited him from remarrying. His children studied abroad and married foreigners. The family consequently felt most comfortable among the foreign community in Mexico; indeed one son had British citizenship and repeat-

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edly prevailed on the British government to help Martínez del Río Hermanos avoid taxation or collect debts from the Mexican government. It is not clear, of course, that family contacts would have prevented the firm's bankruptcy, since many Mexican companies suffered a similar fate. Walker makes a strong case, however, that given the uncertainty and highly politicized economy, those with well-placed relatives had an easier time pulling through.

In the introduction Walker addresses prevalent explanations of Mexico's underdevelopment and finds them wanting. His theoretical discussions are not as lucid as his construction of the case history, however. His refutation of dependency theory is especially strained, since his study shows that competition from foreign manufactures and foreign intervention did hurt Mexican enterprises. His refutation of the cultural explanation is more convincing: Mexican businessmen—or at least the Martínez del Ríos and a handful of their associates—were aggressive, risk-taking entrepreneurs. Their problems stemmed not from incompetence but from the unstable economic climate. The real value of this book, though, is in the richness of detail with which Walker brings to life a facet of early republican Mexico. This will be of interest both to specialists on Mexican history and to those concerned with Latin American development in the nineteenth century.

SILVIA MARINA ARROM
Indiana University

WILLIAM H. BEEZLEY. *Judas at the Jockey Club and Other Episodes of Porfirian Mexico.* Lincoln: University of Nebraska Press. 1987. Pp. x, 179. $19.95.

The search for "Lo Mexicano," the collective Mexican national identity, has all but ignored the great class division in Mexican society and therefore the possibility of cultural schizophrenia. The tension in the Mexican soul between Spaniard and Indian is interesting, but the schism between "los de arriba" and "los de abajo," or the topcats, the rich, and the underdogs, the poor, constitutes the basis of Mexico's two very different cultures: popular or traditional and elite or modernizing. In three superbly crafted and incisive essays, William H. Beezley examines the leisure culture of high society and the traditional culture of "everyday Mexicans" and their interaction and clash (as symbolized by Judas burnings at the elite Mexico City social club, the Jockey Club, in the 1890s) during the age of order and progress under the dictatorship of Porfirio Diaz.

In the first essay, "The Porfirian Persuasion," Beezley re-creates the history of elite sport and recreation in Porfirian Mexico. The prohibition of bull fighting and the adoption of baseball, horse racing, boxing, mountaineering, and cycling reflected the influence of foreigners and the imitative character of the Porfirian persuasion as well as the peace and stability of the Porfirian system, which impelled elite Mexicans to find excitement in the risks of sport in the absence of civil war, revolution, and foreign invasion. At the same time, as Beezley explains in his second essay, "Rocks and Rawhide in Rural Society," rural Mexicans resisted change, adjusted to their environment, were resigned to poverty, and found solace and pleasure in their traditions. As elite Mexicans took up the fad of cycling, recognizing "the importance of progress, speed, and modernization through technology," rural Mexicans cut off one handle on imported plows so they looked like the traditional implement, and, reportedly, one workman loaded a wheelbarrow with bricks, lifted it onto his head, and carried it to the masons. In his third and final essay, "Judas at the Jockey Club," Beezley recounts the origins and practice of Judas burnings as a folk celebration on Holy Saturday preceding Easter. As a popular carnivalesque ritual, Judas burnings turned the world upside down by sanctioning folk (that is, mocking and deriding) humor, providing the illusion of abundance for all through feasting and throwing coins into the mob, and demonstrating social reversal by portraying hated public officials, policemen, and local bigwigs as Judases to be tormented and finally "killed" by exploding fireworks. Beezley argues that Mexicans of all social classes participated in Judas burnings, and even the elite Jockey Club provided Judas effigies for local crowds until the 1890s. Under pressure from newspapers and the city council, the Jockey Club sponsored no more burnings after 1893. "Calm, satisfied, and secure, these Porfirians opposed displays of disorder," writes Beezley. "The destruction of Judas was a traditional, literal exhibition that both the Diaz government and the church saw as a challenge to propriety, with the potential for disorder" (p. 108). The controversy surrounding the Judas burnings at the Jockey Club in 1893 symbolized the growing gap between modern and traditional cultures in Mexico, a gap that contributed to the Mexican revolution after 1910.

Beezley's book is witty and entertaining but also thought-provoking. It is a book that deserves a broader readership than the narrow circle of academic Mexicanists and Latin Americanists. And if the University of Nebraska Press would publish an affordable paperback edition I would join with (I am sure) many others in assigning it as required reading for Latin American survey courses.

THOMAS BENJAMIN
Central Michigan University

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ROMANA FALCÓN. *Revolución y Caciquismo: San Luis Potosí, 1910–1938*. Mexico City: El Colegio de México. 1984. Pp. 306.

This rich, finely crafted study of the north central state of San Luis Potosí constitutes a state-of-the-art contribution to the new regional historiography of the Mexican revolution. Prior to 1968 most writers portrayed the upheaval as a spontaneous, genuinely popular peasant revolt against a backward and abusive hacienda regime. They argued that, although the revolution was initially brutal and chaotic, it ultimately forged a modern nation—more stable, prosperous, and socially just than the Porfiriato. In the present book Romana Falcón places herself in the front ranks of the current generation of "revisionists," capturing the revolution's significance in decidedly darker hues. Although it may have begun as mass mayhem with the active participation of truly popular groupings, the revolution rapidly witnessed the ascendancy of aspiring bourgeois and petit-bourgeois elements. These revolutionary *caciques*—among them veritable regional warlords such as Saturnino Cedillo—often employed traditional patterns of authority, based on patron-client exchanges, to co-opt and manipulate the masses of peasants and workers. Ultimately, in the 1930s, the more independent of these regional power holders were themselves eclipsed—and, if they proved particularly obstinate, like Cedillo, physically eliminated—by the emerging national revolutionary state. An insatiable modern Leviathan, the new state swallowed up regional political configurations, eventually perfecting, in Toquevillian fashion, the formula of political centralization and dependent capitalist development that had begun to emerge under the *ancien régime*.

This revisionist preoccupation with continuity lies at the heart of Falcón's book. Yet unlike most narrowly drawn regional monographs hers has chronological sweep, which permits painstaking documentation of both the disintegration and the overhaul of the state in San Luis Potosí, a process running from the late Porfiriato through Lázaro Cárdenas's regime. Her impressive research rests on public and private archives in Mexico, the United States, and Great Britain.

Falcón appreciates that class analysis alone is insufficient to understand the character and motivations behind the myriad mobilizations that collectively constituted the revolution. Her sophisticated use of clientelist theory and her sensitivity to regional and local nuances of rural society combine with class analysis to produce an ambitious but effective conceptualization of the problem of rural revolt.

In the process Falcón's work deepens our understanding of the pivotal role of the *ranchero* during both the Porfirian and revolutionary periods. On the whole her findings endorse the prevailing revisionist interpretation that these small- and medium-size proprietors, many of whom were beneficiaries of the Porfirian pattern of development, found their main political and economic chance in the revolution. Although her book will receive greatest attention for its meticulous reconstruction of the Cedillo *cacicazgo* during the 1920s and 1930s, the study also breaks new ground with its detailed examination of *ranchero* enrichment through the management of "intervened properties," seized during the 1910s from the old Porfirian oligarchy. Nevertheless, a careful reading of Falcón's work suggests that it may be risky to generalize about *ranchero* status or mentality; the revolutionary potential of these middle strata depended on a whole constellation of local forces. In certain instances *rancheros potosinos* galvanized popular agrarian discontent; in others, they stood directly in the path of agrarian mobilizations.

Many regional studies of the revolution suffer from a failure rigorously to define what they mean by region. Falcón has in part obviated this problem by working on two levels. On one level she has written an institutional political narrative of the state of San Luis Potosí. More important, she has re-created the social history and political culture of southeastern San Luis, focusing on the original Cedillo *cacicazgo* and its environs. Although her study of the phenomenon of *caciquismo* purports to be statewide, relatively few data are presented for localities outside the southeast. Interestingly, Falcón's sensitivity to *cacique* politics in the *huasteca potosina* occasionally extends her analysis to events and actors in neighboring Tamaulipas and Veracruz, suggesting the need for a systematic regional study of the revolution in the northern *huasteca*. Unfortunately, the author's sure grasp of regional geography is not matched by a more detailed map of revolutionary San Luis and its neighbors.

But these minor reservations hardly detract from what is arguably the best regional revolutionary study by a Mexican scholar since Héctor Aguilar Camín's *La frontera nómada* a decade ago.

GILBERT M. JOSEPH
University of North Carolina

THOMAS R. MCGUIRE. *Politics and Ethnicity on the Rio Yaqui: Potam Revisited*. (PROFMEX Monograph Series, number 1.) Tucson: University of Arizona Press. 1986. Pp. xiv, 186. $19.95.

The persistence of the Yaquis of northwest Mexico as a tribal entity has become a topic of growing interest to scholars from a variety of disciplines. They have demonstrated that persistence *through*

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time and at various points *in* time. Working from an anthropological perspective, Thomas R. McGuire seeks to validate that ethnic continuity in the contemporary period but as well to find the as yet elusive answer as to the fundamental reason for that persistence. He succeeds admirably in the first task. But, in the latter purpose, he assembles all of the complicated parts of the puzzle yet leaves the final, critical piece in the box.

McGuire presents the structural and cultural norms of Yaqui society at mid-twentieth century worked out by Edward Spicer and carefully constructs an interpretive framework to account for the deviation from those norms in the 1970s. To do so, he employs the models of several recent anthropologists, including that for the traditional Middle American Indian community. Drawing on a variety of empirical evidence, he concludes that, though partially helpful, none of the models for the persistence of ethnic identity fit the Yaqui reality. In part this results from the failure of such models to link such indigenous communities to regional centers and metropolises. In constructing his own model, he relies heavily on the role of those external forces, to the point, at times, of losing the balance to his argument in a morass of marginal details.

McGuire's model of polity and ethnicity for the Yaquis consists of four basic premises. The existence of a corporate Yaqui polity (acquiring a growing measure of official legitimacy since 1940) serves to base group membership on ascription (lineal descent), through a definitive classification of persons by tribal officials. That membership by descent, in turn, fosters the disjunction of wealth, power, and prestige since membership is not judged on performance in "things Yaqui." This relative isolation of status dimensions, one from another, relieves Yaqui ritual organization from the pressures of poverty and wealth and of external economic and political forces. Yaqui ritual, then, by its ceremonial openness, by its attractiveness to both Yaquis and Mexicans alike, serves to maintain ethnic boundaries between them.

In a concluding chapter that ably assembles all of these analytical pieces, McGuire notes the role of Yaqui adaptability in the persistence of the tribe's identity in the face of changing historical circumstances. Primary to that adaptability, he concludes, are the existence and operation of the Yaqui corporate polity and the reliance on descent as the criterion for membership in it. As he acknowledges—"verification . . . must rest on a close examination of history" (p. 162)—the historical origins and development through time of those two keys to the puzzle of Yaqui persistence that he has so ably demonstrated are missing. That is the final piece that another study will have to fill in.

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LINDA NEWSON. *The Cost of Conquest: Indian Decline in Honduras under Spanish Rule.* (Dellplain Latin American Studies, number 20.) Boulder, Colo.: Westview. 1986. Pp. 375. $28.50.

Linda Newson's careful research and creative organization and analysis have produced an impressive volume on the colonial demographic history of Honduras. Together with her even more recent work on Nicaragua, it fills large gaps in our knowledge of Spanish Central American history. Newson joins a distinguished group of mostly British historical geographers (David Browning, Carolyn Hall, and W. George Lovell) and the Spanish ethnohistorian Elias Zamora who have written monographs on single Central American countries (once individual jurisdictions of the colonial Audiencia of Guatemala) or regions within a particular country.

Newson builds on the work of Robert S. Chamberlain while enriching the introductory chapters with descriptions of Indian civilization at Spanish contact. Dividing Honduras into three geographic zones, the author finds that more sedentary, hierarchical chiefdoms existed in the western and central regions that form part of the border area of southeastern Mesoamerica, whereas nomadic, more egalitarian tribes inhabited the east. Topography, natural resources, climate, and proximity to trade routes resulted in more complex, populous societies in the western and central regions and, by contrast, less complex and smaller populations in the east.

These pre-Hispanic patterns influenced Honduran socioeconomic and demographic development in the critical conquest and early settlement period from 1522 to 1550 as well as from 1550 until independence in 1821. Areas of dense Indian population or alluvial gold deposits attracted Spaniards. Understandably, Newson finds it difficult to document precisely where Indian enslavement was most destructive prior to its effective end in the early 1540s, and gold was to be found in almost every river in the province. Because Indians were enslaved and exported, subjected to Old World diseases, and forced to labor as miners and bearers (combined with forced change of climatic zones), Indian populations declined drastically in all regions.

Newson reasonably estimates a total aboriginal population of 800,000, with 600,000 in the western and central regions, and the remaining 200,000 in

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eastern Honduras. By ca. 1550, because of conquest and heavier colonization, the Indian population of the western and central regions had declined by 95 percent to only 32,000; in the eastern region, less affected by colonization with the exception of Olancho, Newson, admitting uncertainty, estimates a 50 percent loss to approximately 100,000. Between 1550 and ca. 1700 Indian population continued to decline in the central-western (51.4 percent) and the eastern (68 percent) regions, reaching its nadir by the end of this period in the former, more Hispanicized area. Between ca. 1700 and the early nineteenth century, Indian population, in spite of widespread miscegenation, recovered and doubled in the west and center; however, it continued to decline, overall, in the east. Because of outside encroachments (Spanish colonists and missionaries as well as Zambos-Mosquitos) resulting in increased impact of Old World diseases, the decline only occurred in the "area outside Spanish control" (table 26, p. 330); the far smaller eastern Indian population, under Spanish control, actually increased by 52.9 percent. Newson shows that just prior to independence only a few thousand more Indians survived in the western and central regions (32,635) than in the east (30,057). Regional variations in the distribution of attractive natural resources within the more colonized western and central regions resulted in varying rates of Indian survival. Newson concludes that "Indian survival was favored in more complex and productive societies and where few resources existed to attract conquistadors and colonists" while, "in less well-developed societies and where there were abundant natural resources, Indian survival was more problematic"(p. 336).

Newson has made a major contribution to Spanish Central American demographic history. Undoubtedly, this study will stimulate considerable discussion and inspire others to research themes introduced in this work.

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E. BRADFORD BURNS. *Eadweard Muybridge in Guatemala, 1875: The Photographer as Social Recorder*. Photographs by EADWEARD J. MUYBRIDGE. Berkeley and Los Angeles: University of California Press. 1986. Pp. viii, 136. $35.00.

Rarely are artists, photographers, or historians invited to witness and record a revolution. Even S. M. Eisenstein had to re-create fictively the epic events of the October revolution. How fortunate, then, the British expatriate photographer Eadweard Muybridge was to have been invited to record for posterity the largely silent, coffee-based revolution of nineteenth-century Guatemala. Through Muybridge's lens, and E. Bradford Burns's deft prose, we gain a unique perspective on the key period of change for all of Central America, the liberal revolution that both confirmed and deepened coffee's predominance within local agrarian society.

Muybridge was not commissioned to record coffee's ascendancy alone. Financial interests in the United States, particularly those of San Francisco, Muybridge's adopted home, were to be impressed by visual evidence of Guatemala's "progress" under Liberal rule. They were to be further enticed to invest in the coffee trade itself, one might suppose, given the photographer's "seedling to harvest to shipment" portfolio documenting all aspects of this key activity. Burns provides a very elegant commentary on each set of photos, based in part on his well-known work *The Poverty of Progress* (1980) but also employing travelers' accounts to good effect, such as that of the coffee heiress Helen Sanborn. Moreover, each photo's caption adds further to the historical, contextual placement of the image.

It is impossible to do justice to the beauty and historical worth of this collection of images in such a brief review as this. However, there are three themes that might be highlighted for prospective readers. The first series of photos shows a coffeeless, or precoffee Guatemala whose colonial, "timeless" nature is repeatedly alluded to by Burns. Timeless or not, the images depict a radically different cultivation system and relationship between land and people than those brought on by coffee—ones we find difficult even to imagine throughout the region today. Second, the truly colonial, oppressive nature of coffee-plantation society in Guatemala is revealed repeatedly, whether in the obvious dichotomy of upper (European-like) and lower (Indian-like) classes, habits of dress (the stiff-collared wealthy versus the traditionally attired and bare-breasted laborers), or the sexual division of tasks among the laborers themselves (the men clearing the forest, planting, and processing, the women picking the ripe beans). The visual impression is more akin to that of an English colonial plantation in Africa or India than to that of triumphant liberal revolution. Finally, the several-meters-high *arábigo* variety of the coffee bush and the use of interspaced shade trees left in place during the clearing of the forest constitute important evidence of the diffusion, from Costa Rica, of a pan–Central American cultivation pattern during the nineteenth century. Soon this too will likely be difficult to remember, as difficult as the precoffee landscape, given the massive replacement of the old bushes with genetically improved dwarf varieties requiring no shade in our own times.

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Burns and the University of California Press are to be congratulated for rescuing Muybridge's work from oblivion. With so many volumes appearing recently that claim to resolve all of Central America's problems, or to assess blame for them, it is heartening to find an interest in the region more for its enduring attractions than for its contemporary agonies. This work offers a unique perspective on nineteenth-century Guatemala and Central America, a period both heir to a rich colonial past and a major source of today's difficulties. It should be required reading for those interested not only in Guatemala or Central America but in the agrarian transformation of nineteenth-century Latin America as well.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

ALFRED LORENZER, editor. *Kultur Analysen: Psychoanalytische Studien zur Kultur.* Frankfurt a/M: Fischer. 1986. Pp. 395.

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CLAUDIO POGLIANO, Scienze della natura e scienze dell'uomo. MARINA BONIFETTO, I linguaggi della politica. MARIO RICCIARDI, Arte e Letteratura nella società politica. SILVIA ROTA GHIBAUDI, L'utopia e l'utopismo. CARLO MARLETTI, Emancipazione e liberazione. AURELIA CAMPARINI, Donna, donne e femminismo. LUCIO LEVI, Il federalismo. LUIGI BONANATE, Guerra e pace. MANUELA VALENTI, Charles Darwin. PIER PAOLO PORTINARO, Max Weber. SILVIO STELLA and CLAUDIO CASSARDO, Sigmund Freud. ICILIO VECCHIOTTI, Mohandās Karamchand Gāndhī. MARIO RASETTI, Albert Einstein.

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ARTICLES

TO THE EDITOR:

The participants in the *AHR Forum* on abolitionism (*AHR*, 92 [1987]: 797–878) seem to believe that their descriptions of the connection between capitalism and abolitionism are mutually exclusive. Why can't they be complementary?

Sensitivity to the plight of strangers might have been a necessary precondition for abolitionism without having been a sufficient cause. Sufficiency might have required that the lower orders closer to home could *not* be an object of similar philanthropy (because to change conditions closer to home would be to undermine the abolitionist's own class).

Similarly, abolitionism might have depended on a notion of individual will that inflated the will's power (as Thomas Haskell suggests); but this notion might have contributed less to the abolitionist outlook if abolitionism had not also served as a *quid pro quo* to wage laborers: if the laborers would refrain from depending on unions or other forms of collective organization, their employers would see to it that workers could never be forced into a less benign form of dependency (then existing only in a faraway place, but capable of spreading closer).

Indeed, in the first number of the *Liberator*, William Lloyd Garrison described as "criminal" the efforts "to array [our mechanics] under a party banner."

Furthermore, markets and care for strangers might also be related to each other with causality operating in a direction opposite to that described by Haskell. Interest in things far away and strange (exhibited, for example, by the Crusades) might have been a precondition for the rise of the market.

Finally, capitalism and abolitionism might be most significantly related to each other through a third factor—such as an accelerated rise in the population of the Western world.

RICHARD JOFFE
Maplewood, New Jersey

TO THE EDITOR:

Reading the learned articles of David Brion Davis, John Ashworth, and Thomas L. Haskell (*AHR Forum*, *AHR*, 92 [1987]: 797–878), I get the impression that the controversy over the relationship between capitalism and abolitionism serves primarily as an occasion, a battleground if you will, for a war that is actually being waged over the concept of hegemony.

The choice of battlefield is appropriate enough, given that hegemony made its debut in the discourse of American historiography mainly through its creative deployment by Eugene Genovese in *Roll, Jordan, Roll* (1974). Ever since John Cammett's *Antonio Gramsci and the Origins of Italian Communism* (1967) spurred interest in the concept and its originator, both have become increasingly studied, diffused, and celebrated. The concept alone now provokes book-length studies, such as John Bocock's *Hegemony* (1986). Since what is at stake in a war over theory may be of immense and long-range importance, I would like to make a few remarks on the Haskell-Davis debate.

As Davis points out, the "concept of hegemony is easily discredited by misconstruction or misunderstanding" (p. 807). In trying to discredit hegemony, Haskell piles up several misconstructions and misunderstandings. Nor are Davis's arguments completely convincing, since he ties his defense to the unprovable concept of "self-deception" and to the implausible notion of "unconscious intention." In history, one finds unstated intentions, unintended results, even actions done unconsciously. But what is

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an "unconscious intention" except a contradiction in terms? Neither Haskell nor Davis is clear on this.

Equally unclear is Haskell's distinction between "hegemonic" and "pluralistic" societies (p. 834). Such a distinction makes no sense within Gramsci's theory. It implies that hegemonic refers only to some form of extraordinarily repressive or monolithic regime. It also implies that a pluralistic society is not itself held together by hegemonic forces and is thus equally subject to analysis by the concept. It is precisely the potency of hegemony as an analytic tool that explains its prevalence in the discourse of the "pluralistic" West.

In the first seven pages of his article, Haskell hammers away, repeatedly pounding on phrases such as "hegemonic mode," "hegemonic argument," "hegemonic control," "hegemonic implications," "the hegemony model," and "under the banner of hegemony" (pp. 829–36). At the climax of this beating on "the frail reed of hegemony" (if it is so frail, why hit it so hard?), he strikes a sour note with the phrase "the Gramscian alchemy" (p. 834). Although Gramsci has often been accused of Hegelianism, Stalinism, Trotskyism, Maoism, materialism, nihilism, totalitarianism, and liberalism, this is the first time he has been implicated in alchemy!

Haskell is not beyond trying his own hand at magic. He conjures up a metaphor (out of physics) to dismiss the concept of hegemony. He complains that, "like dry ice, hegemony always tends toward sublimation, becoming merely a diffuse aspect of the human condition rather than a distinct feature of particular societies that one could ever point to in explanation of specific events and actions" (p. 835). Might one not dismiss gravity with the same metaphor? The power of gravity tends to "evaporate" the further one moves away from the earth's center; and gravity, too, is "diffuse" or universal, rather than "particular."

Haskell's strategy involves turning Davis's *Problem of Slavery in the Age of Revolution, 1770–1823* (1975) into a flagship of the hegemonic model. He would like to reduce this 750-page volume to the argument that "abolitionism is largely to be understood as an instrument of hegemonic control" (p. 831). Davis objects to any such reduction (p. 798). Does Haskell imagine that if he can sink Davis he will win the battle and be able to celebrate a victory for his own concepts of "markets" and "convention"? Whatever the outcome of this encounter, it will hardly sweep away the theory of hegemony.

Beyond the limited configuration that the concept has assumed in this forum and in T. J. Jackson Lears's article ("The Concept of Cultural Hegemony: Problems and Possibilities," *AHR*, 90 [1985]: 567–93), hegemony is being deployed in multiple ways in contemporary discussions of politics, culture, and the relation between the two. (See, for example, Christine Buci-Glucksmann's *Gramsci and the State* [1980] and Joseph Buttigieg's "Gramscian Presence in American Criticism" in *Socialism and Democracy*, Fall–Winter 1987.) In Gramsci's own work, hegemony is polyhedral. (See Joseph Femia's *Gramsci's Political Thought* [1981], Walter Adamson's *Hegemony and Revolution* [1980], and my reviews of these books in *Italian Quarterly*, Summer–Fall 1984.) Then there are also the "new and improved" versions introduced by interpreters such as Ernesto Laclau and Chantal Mouffe in *Hegemony and Socialist Strategy* (1985). The sheer volume of writing (more than 700 items in English alone) indicates that Gramsci and his theory will not be so easy to dismiss, sublimate, or sink.

In the meantime, Gramsci may, as Haskell says, provide "a feather pillow, perfect for catching falling Marxists" (p. 834). I think it equally true that Gramsci provides a safety net of coherence and faith for intellectuals whose liberal ideals have been battered by the experiences of Vietnam, racism, inequality, the imperialist role of the United States in the Third World, and the impoverishment of life and culture in this era of giant corporate powers, rampant egoism, and unrestrained greed.

WILLIAM HARTLEY
Chicago, Illinois

PROFESSOR HASKELL REPLIES:

Hegemonic interest does not, I think, tell us much about the abolition of slavery. To the extent that David Brion Davis thinks it does, our views are not compatible, and well-intentioned efforts like those of Richard Joffe to find common ground are not likely to succeed. Still, for reasons other than the ones Joffe gives in his letter, there is a significant sense in which he is right to say that my views complement those of Davis. Certainly, Davis's stress on interest and mine on convention should not be mistaken for mutually exclusive approaches: I think we both understand that conventions sometimes flourish because powerful interests sustain them, and that interests sometimes become powerful because they are reinforced by convention. Moreover, although Davis has stressed hegemonic interest, that is not the only explanatory motif to be found in *The Problem of Slavery in the Age of Revolution, 1770–1823* (1975). My way of accounting for the concomitance of capitalism and antislavery (by relating the new moral sensibility to an outward shift of causal conventions produced by the market) is, in one sense, an alternative to Davis's interpretation, but in another sense I have always understood my approach as an effort to bring out and develop more systematically than Davis himself did a theme that is at least nascent in his own text. My aim has not been to

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negate his interpretation but to strengthen a revised version of it by showing that the connecting link between capitalism and the humanitarian sensibility need not be mainly, or even importantly, a matter of class interest. At one level, then, our viewpoints are indeed complementary, and I would not be surprised if that complementarity loomed larger in the years ahead, as both of us come under attack from historians convinced that there is no substantive connection at all between capitalism and antislavery.

Turning now to William Hartley's letter, I have no doubt that the adherents of the hegemonic interpretation are just as able and numerous as he says they are. I am pleased to have such worthy opponents and glad to have Hartley's bibliographical guidance to their work, but my question still stands: are they wise to rest their faith on the feather pillow (or "safety net," as Hartley prefers to call it) of hegemonic theory? The way to assess the adequacy of the theory is to apply it to particular historical episodes and contrast it with alternative interpretations. This Hartley's letter does not do. Apart from glowering in my direction and airily dismissing Davis's pivotal notions of self-deception and unconscious intention, Hartley offers no assessment of the rival claims that have been put forward in the current controversy. Hartley's principal concern seems to be to put some distance between what he takes to be the main body of hegemonic theory and the interpretation of antislavery advanced by Davis, as well as to assure us that, no matter what happens to Davis, the heavy guns of the hegemonic persuasion remain intact, ready to fight again another day. In the meantime, Hartley is content for the hegemonic armada to steam over the horizon, abandoning Davis to his critics.

The fate of the hegemonic interpretation is less interesting to me than the origins of the humanitarian sensibility, and so I cannot agree with Hartley's contention that the debate is "primarily" about hegemony, but it is not surprising that he and other adherents of the hegemonic view feel that they have a stake in the outcome. Given the high stakes of the debate, Hartley's abandonment of Davis is a curious strategy. Can supporters of the hegemonic interpretation really do without self-deception and unconscious intention, or functionally equivalent substitutes for these ideas? And can the followers of Gramsci really afford to admit that hegemony operates everywhere, all the time—that hegemony, instead of being a historically and culturally specific phenomenon one might hope someday to overcome, and to which one might point in explanation of specific events and choices, is instead, like gravity, a virtually universal aspect of the human condition, a condition of earthly existence itself? I would have thought that the answer to all of these questions would have to be no, but it is Hartley's allies who need to pass judgment on his strategy, not me. Certainly, if the hegemonic perspective tells us no more about the abolitionists' motivation than the presence of oxygen tells us about a particular case of arson, or gravity about the collapse of a particular building, many of us will feel that hegemony does not explain much. Hartley seems not to understand that by embracing the idea that hegemony is as pervasive as gravity he makes it relevant to everything but incapable of explaining anything in particular. Like an architect who would explain the collapse of a building by saying that "gravity exerted a downward force," or an arson investigator who announces that "oxygen was present," Hartley is not in a position to tell us what we need to know.

As I see it, although the notion of self-deception is finally incapable of yielding a satisfactory formulation of the relationship between capitalism and antislavery, Davis resorted to it out of a commendable scrupulosity. He faced a problem of explanatory adequacy: eager to avoid the implausible undertones of conspiracy and hypocrisy that marred Eric Williams's interpretation but convinced, like Williams, that the concomitance of capitalism and antislavery was not merely coincidental, Davis strove to do justice both to the humane intentions of the reformers and to the hegemonic consequences (especially the legitimation of free labor) that he saw issuing from their actions. Self-deception and the less-than-conscious form of intentionality that it implies provided a solution to this problem by rendering intelligible an otherwise contradictory pair of claims: that the abolitionists were sincerely humane yet systematically pursuing the interest of their class. Davis's solution is quite possibly the only workable one, as long as one clings to the widespread assumption that the concept of interest supplies the only possible linkage between capitalism and the new moral sensibility, between "base" and "superstructure." The problem Davis grappled with is one of the thorniest in contemporary social theory, and his treatment of it is far more subtle than that of Michel Foucault, for example, who encountered the same problem in *Discipline and Punish* (1977) but merely finessed it. As the philosopher Charles Taylor said in his trenchant criticism of Foucault, "purposefulness without purpose requires a certain kind of explanation to be intelligible." "The undesigned systematicity [or pattern of consequences] has to be related to the purposeful action of agents in a way that we can understand ... The reason for this requirement is that the text of history, which we are trying to explain, is made of purposeful human action. Where there are patterns in this action that are not on purpose, we have to explain why action done under one description on purpose also bears this other, undesigned description. We have to show how the two descriptions relate. A strategic pattern

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cannot just be left hanging, unrelated to our conscious ends and projects ... It is certainly not the case that all patterns *issue* from conscious action, but all patterns have to be made *intelligible* in relation to conscious action." (Taylor, "Foucault on Freedom and Truth," *Political Theory*, 12 [1984]: 169–70).

Few practitioners of the hegemonic interpretation have been as sensitive as Davis to the need for an intelligible relationship between intentions and consequences. How would Hartley, committed as I suppose any Gramscian must be to the indispensability of the concept of class interest, deal with Davis's problem? Without self-deception and unconscious intention, or some close (though perhaps tacit) substitutes, how could he even begin to render intelligible the relation between the reformers' benevolent consciousness and the pattern of hegemonic social consequences that issued from their action? Surely, he does not mean to regress all the way back to the view that the humanitarian discourse of the reformers was just a cloak for the conscious pursuit of class interest. If the adherents of the hegemonic interpretation are to make good on their claim to have superseded the earlier and candidly economistic school of Marxian thought that Eric Williams exemplifies, I do not see how they can avoid relying (at least tacitly) on some soft form of intentionality similar to that employed by Davis—similar, and therefore vulnerable to all the same objections from my standpoint, which locates the linkage between "base" and "superstructure" in a market-triggered shift of conventions, rather than in interest, and which assigns the hegemonic outcome of reform to the morally neutral realm of unintended consequences, rather than to unconsciously intended ones. But the ball is in their court. Theirs is a rich and resourceful interpretive tradition, and I am willing to be shown the error of my ways.

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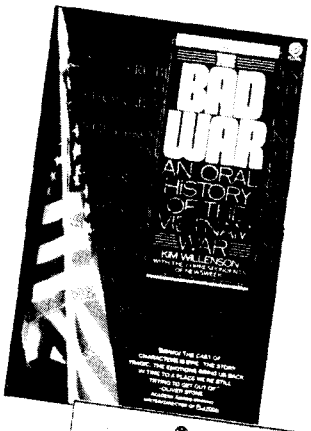
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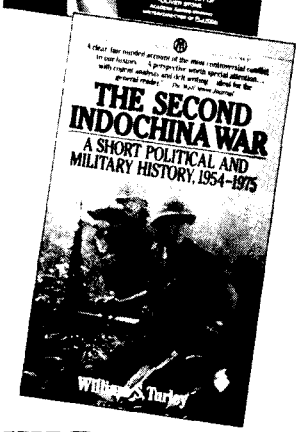
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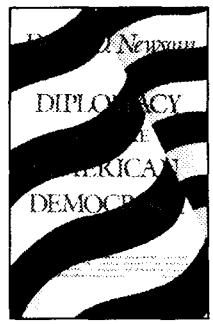
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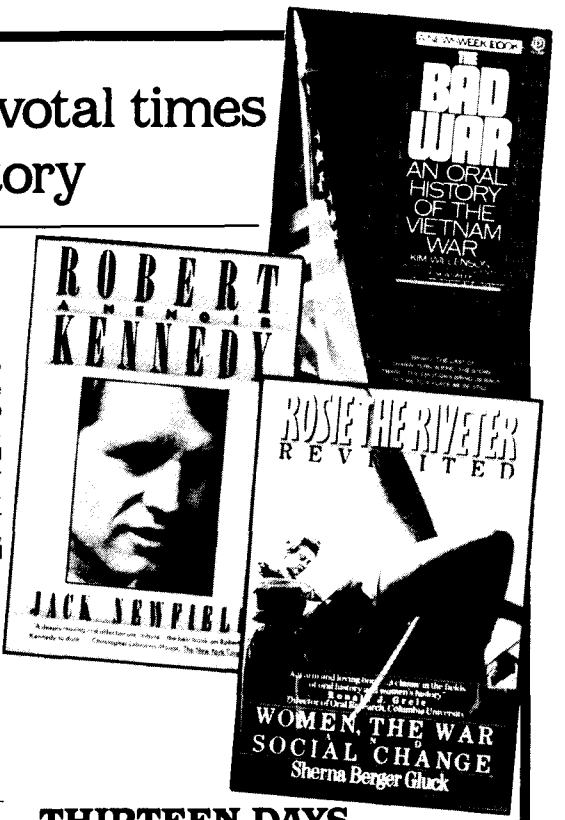
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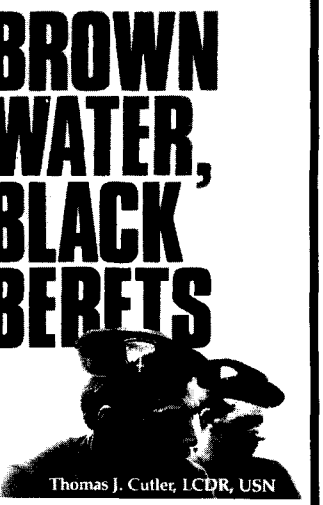
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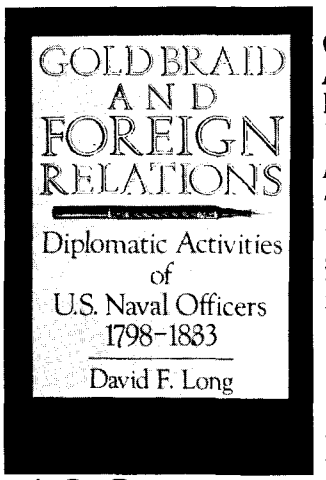
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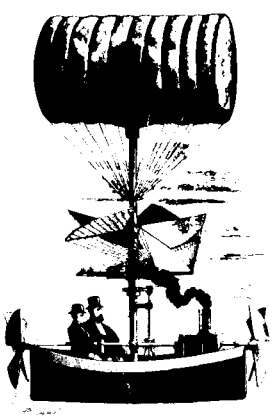
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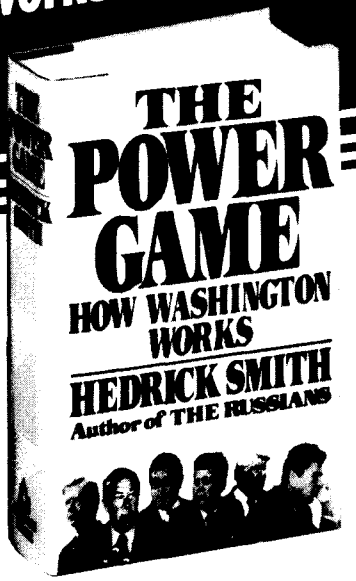
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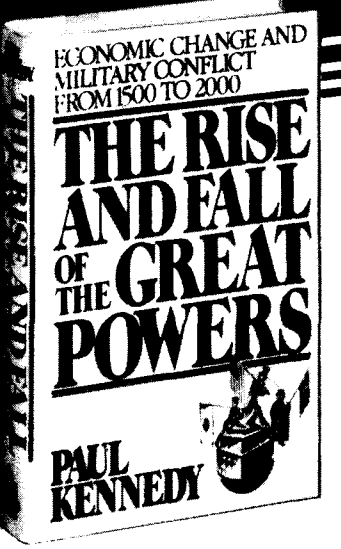
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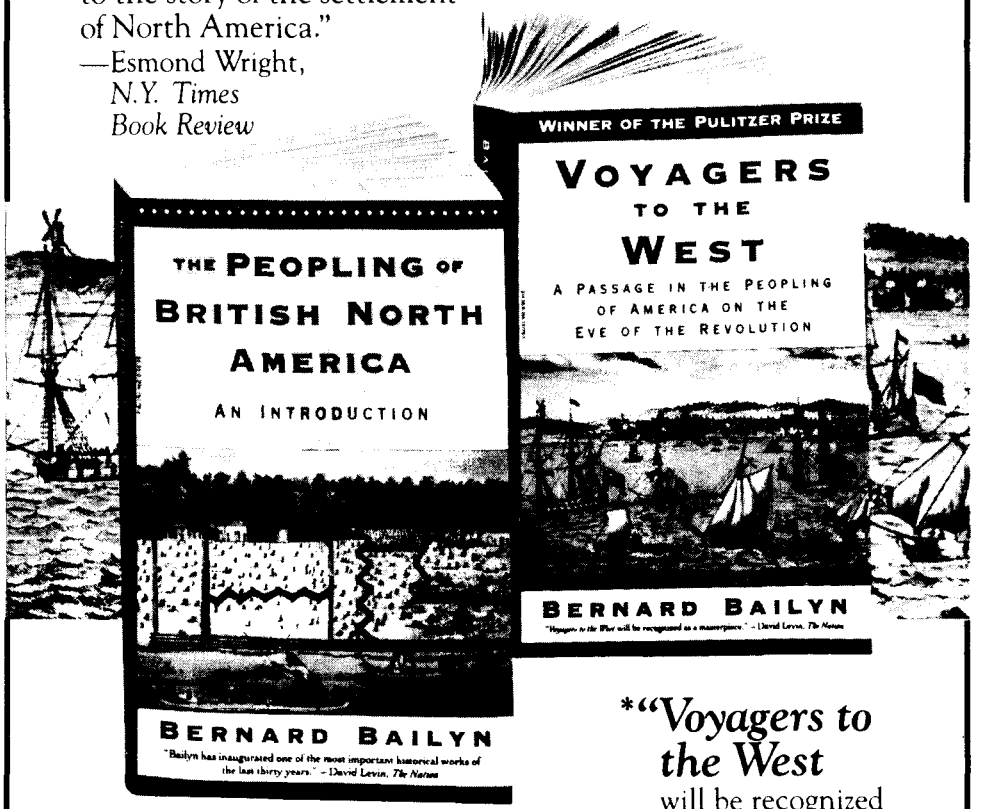
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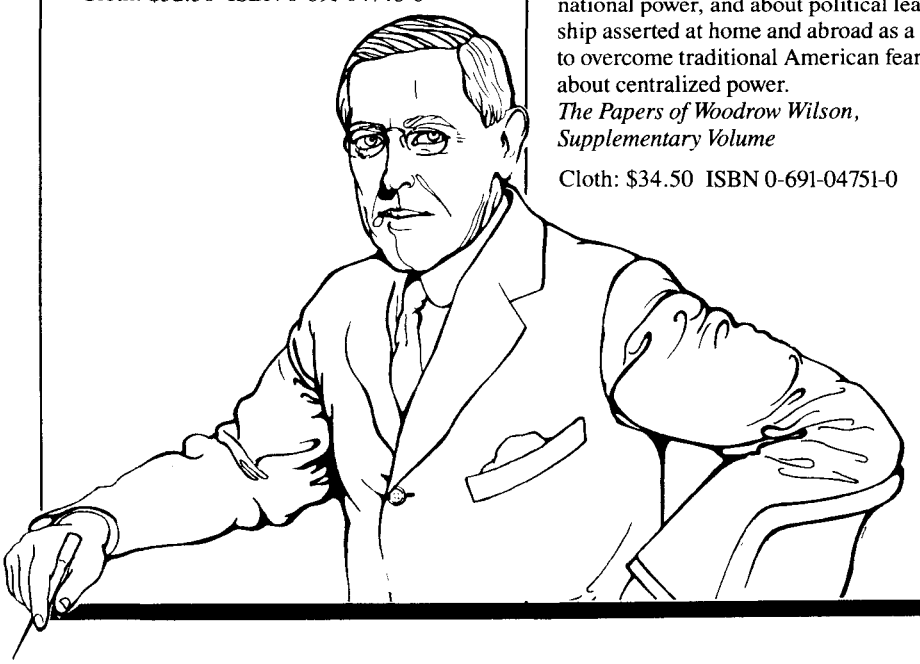
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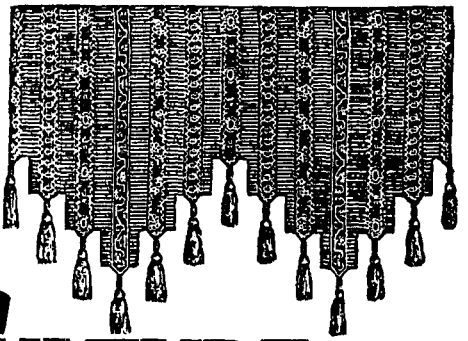


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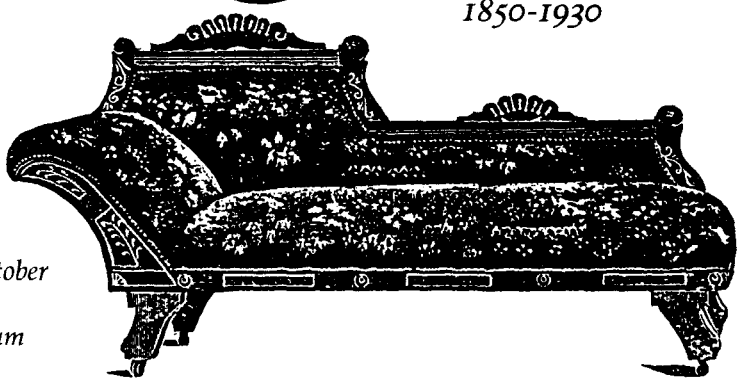
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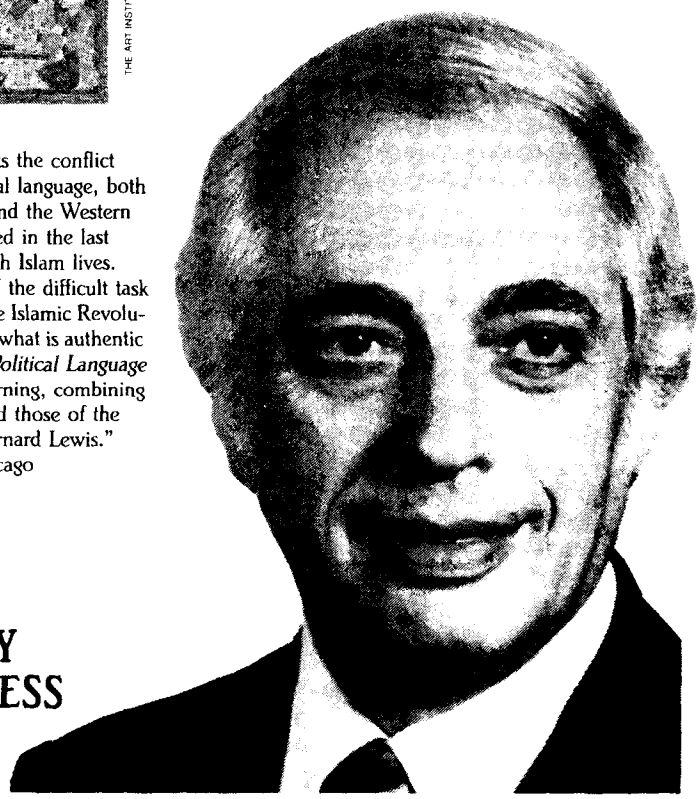
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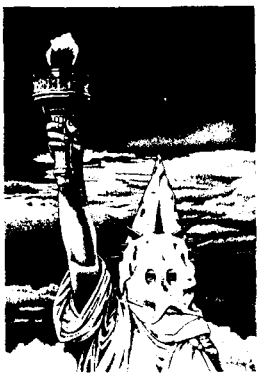
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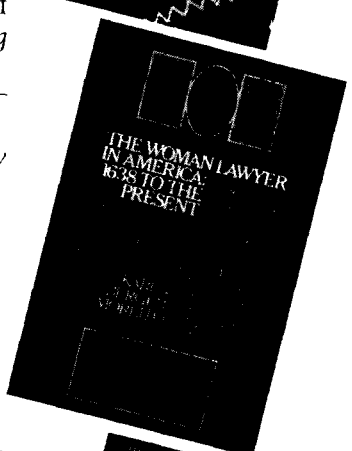
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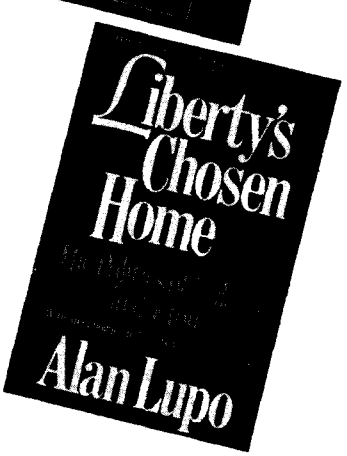
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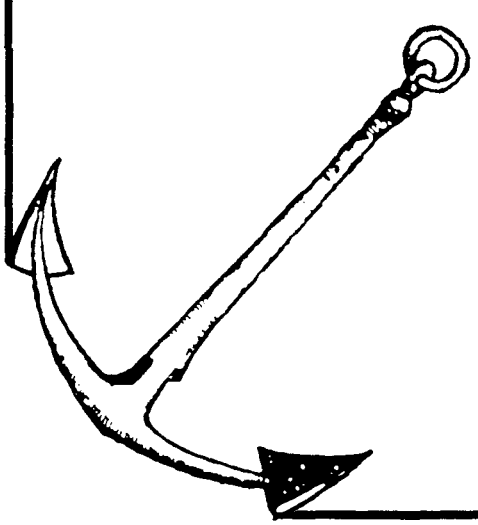
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Index of Advertisers

American Historical Association	42–47
Beacon Press	38
Cambridge University Press	3–6
Columbia University Press	41
Cornell University Press	17–19
Walter de Gruyter	13
The Free Press	9
Harvard University Press	Cover 3
Indiana University Press	8
Johns Hopkins University Press	10
Krieger Publishing Company	12
Louisiana State University Press	14
MIT Press	39
Naval Institute Press	16
New American Library	7,11
Oxford University Press	Cover 2
Princeton University Press	24–27, Cover 4
Random House	21
Southern Illinois University Press	15
The Strong Museum	28
Syracuse University Press	40
University of California Press	29–31
University of Chicago Press	32–35
University of North Carolina Press	37
University of Queensland Press	36
University of Wisconsin Press	20
Vintage Books	23
Yale University Press	22

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